



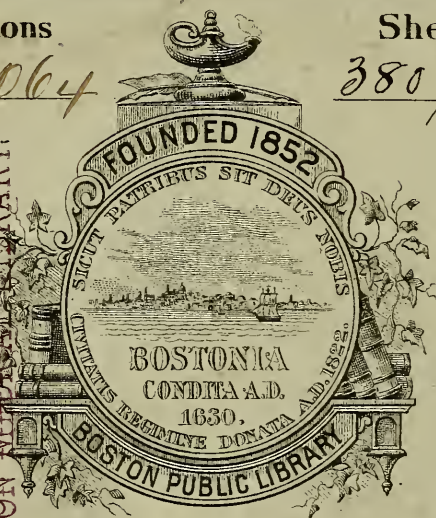
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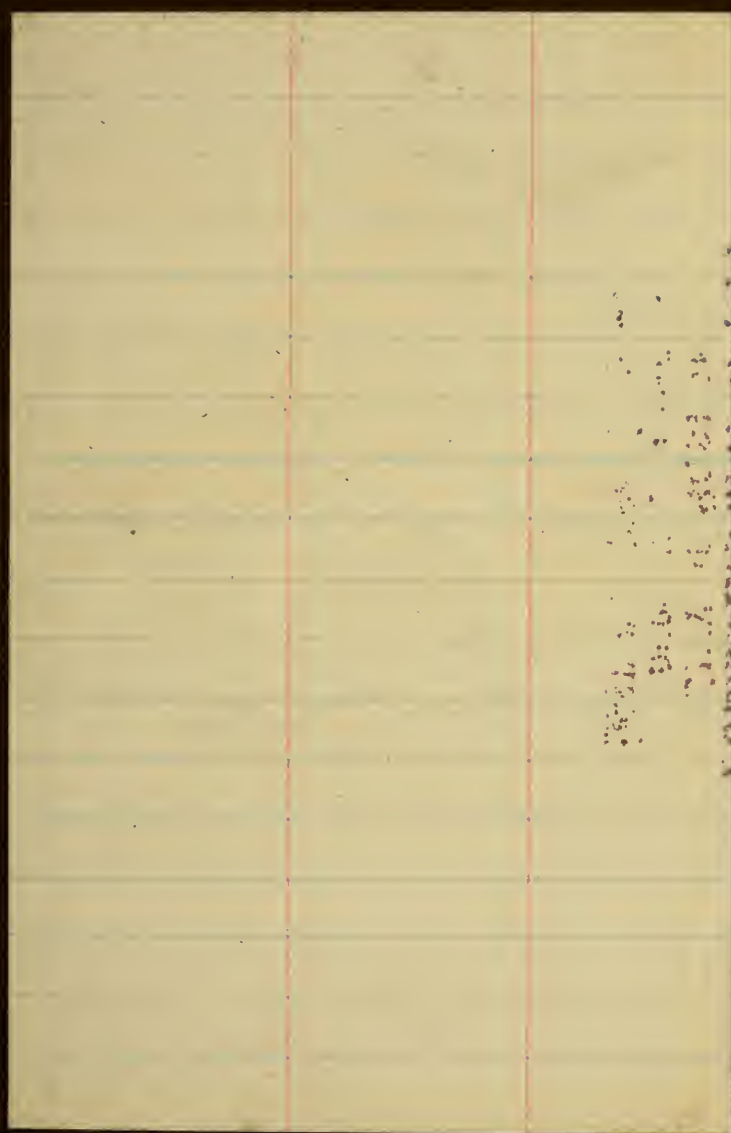
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THE ABOLITION OF
ZYMOTIC DISEASES.

*THE ABOLITION OF
ZYMOTIC DISEASES*

AND OF OTHER SIMILAR ENEMIES
OF MANKIND.

BY

SIR THOMAS WATSON, BART., M.D., F.R.S.



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Convinced of the truth and value of the facts collected in these Essays, he feels that, of all the efforts of a long professional life, they constitute his best contribution to the sanitary welfare of his fellow countrymen.

It is gratifying to him to notice that a bill has just been introduced into Parliament, following closely the lines pointed out in the last of the Essays, for removing the only objection which can reasonably be made against that most precious gift to mankind—vaccination.

July, 1879.



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I.

THE ABOLITION OF ZYMOTIC DISEASE.



THE ABOLITION OF ZYMOTIC DISEASE.

THERE is a very remarkable group or family of bodily diseases which a theory respecting their nature has told us to call *zymotic*, and about which it is of vast importance that the public, no less than the medical profession, should possess the fullest attainable knowledge.

These diseases are distinguished by the following characters. They are all of them febrile diseases. They all run, naturally, a definite course, in definite though different periods of time. They all present, during some (usually definite) portion of that course, certain distinctive spots, markings, or eruptions on the

surface of the body. As a rule, broken and proved by rare exceptions, they occur once only in the same person ; in other words, one visitation of the same disorder protects the subject of it, for the most part, against the recurrence of that disorder. Lastly, they are communicable from person to person by contagion, and, as I venture to maintain, arise in no other way ; and this quality, with their non-recurrence, forms the key to their supreme interest.

It is by the *combination* of these several features that the group is sharply marked off from other diseases which present some, or one, of them only ; from simply contagious febrile diseases, therefore, such as erysipelas, puerperal fever, pyæmia, and indeed from all other known human maladies.

They are not numerous, these zymotic diseases. There are not more than nine or ten of them. Small-pox, chicken-pox, typhus fever, typhoid or enteric fever, scarlet fever, the plague,

measles, hooping-cough, mumps—these belong to, and I think constitute, the group of diseases now to be considered. Few in number as they are, the group is fearfully destructive to human life, and to a still much greater extent prolific of human suffering, misery, and want. Several of them are, however, so familiar to almost every home as to be reckoned among the inevitable ills and perils of childhood; and hence, perhaps, the perception of important lessons which are furnished by the attentive study of them collectively has been obscured. Even of those which are mainly incidental to the earlier periods of life, some are justly held in the utmost dread by parents and nurses.

As life springs only from preceding life—as, according to the verdict of exact scientific experiment, there is no such thing as spontaneous generation—so, under the testimony of long and wide experience, there is, nowadays at least, no spontaneous origin of any of these

specific disorders. It is an axiom not confined to the art of medicine that prevention is better than cure. Only of late years have we fully acknowledged that prevention is more easy also than cure. Let the cause of any disease be clearly discerned, and seized upon, impounded, and destroyed, and its prevention is achieved. In these serious and procreant disorders, happening, if at all, once only in a lifetime, the discovery, combined with the arrest, of their several producing causes, is equivalent to the possibility of their total abolition.

That the diseases of which I am treating are all of them contagious, and have now no other origin than contagion—that they never spring up, in our time, *de novo*—this is the main point which in this paper I shall endeavour to prove. If I succeed in my endeavour, I shall next insist that, by the enactment and rigid enforcement of judicious sanitary laws, these terrible diseases, with their terrible con-

sequences, may finally be banished from this island.

The steps towards this most desirable issue require the co-operation of an enlightened public intelligence with the dictates and efforts of medical science ; and this is my reason and excuse for thus addressing myself, in popular language, to the general reader, and not exclusively to the members of my own profession.

For what I am about to say I can claim no originality, nor shall I scruple to quote, when it serves my purpose, words which I may myself have used on some former occasions. I must premise also that all disorders which are contagious, or "catching," are transmitted from person to person by mutual touch ; or by particles of matter floating in the air or adhering to clothes, bedding, walls, or furniture, and so brought near to, or in contact with, the body of the recipient of the disease. Such particles, many or few in number, constitute its *contagium* ;

more popularly they are called its germs, or, in plain and more accurate English, its seeds ; and each disease in the group has its own proper and peculiar seed.

In offering some desultory comments upon certain of these disorders, I shall begin with small-pox, which is not only the most formidable of them all, but also a type or representative of the characteristics of the whole group. It is a malady which could scarcely be mistaken for any other ; the very odour of the sick person's body is distinctive of it. Its horrible aspect, disfiguring consequences, and fatal tendency are so strongly marked that its presence has always been watched with affright by mankind in general, and with intense interest by the philosophic physician.

In the acme of the disease, when it is severe, the whole surface of the body is studded with a vast multitude of little pustules. A minute portion of the matter contained in any one of

these pustules, just so much as may suffice to moisten the point of a fine needle, is inserted, we will suppose, beneath the skin of a healthy man who has not been near the sick man. What follows this engrafting? Nothing, apparently, for several days; but then febrile symptoms burst forth, and by-and-by a crop of pimples appears, sprinkled over the skin, and these gradually ripen into pustules precisely like that from which the engrafted matter was taken.

The very same phenomena ensue when a healthy man enters the chamber of a person ill of the small-pox, and breathes for a certain time an atmosphere tainted with the emanations from his body.

The points to be noticed here are, (1) the manifest introduction of the virus into the bodily system; (2) its dormancy for a while—the occurrence, in medical language, of a period of *incubation* or *hatching*; (3) the breaking out, at length, of a disease identical in its symptoms

and in its character with that of the first of the sick persons; and (4)—most surprising of all—the enormous increase and multiplication of the poisonous matter.

The whole process is in striking analogy with the growth of wheat in a field, or of other grain. We have the visible and tangible seed, the manifest sowing, the hidden germination, then the outgrowth and efflorescence, the ripening, the mature seed-time, the reproduction manifold of the original specific seed—every stage in the process of development occupying a definite period of time.

How this dire disease first arose among men it is difficult to conjecture. It has been supposed that it may have been originally derived from some disease in the camel. Its history leads to the settled belief that, while most persons are readily susceptible of it, it never occurs now except from contagion. It does not appear to have been known in Europe till the

beginning of the eighth century. No mention of any such distemper is to be found in the Greek or Roman authors of antiquity. Now whatever may have been the deficiencies of these ancient physicians, they were excellent observers, and capital describers, of disease; and it seems to me scarcely possible that a disorder so diffusive, and marked by characters so definite and conspicuous, should have escaped their notice, or if known should have been obscurely portrayed in their writings.

On the other hand, Mr. Moore, in his learned and interesting *History of Small-pox*, has shown that it prevailed in China and Hindostan from a very early period—even more than a thousand years before the advent of our Saviour. That it did not sooner make its way westward into Persia, and thence into Greece, may be attributed partly to the horror which the complaint everywhere inspired, and the attempts which were consequently made to check its progress by

prohibiting all communication with the sick ; partly to the limited intercourse which then took place among the Eastern nations ; but principally to the peculiar position of the regions through which the infection was distributed, separated as they were from the rest of the world by immense deserts and by the ocean.

The disease is said to have broken out in *Arabia* at the siege of Mecca in the year in which Mahomet was born, *i.e.* in the latter half of the sixth century. It was widely propagated by his wars, and by those of the Arabs afterwards ; and it is generally believed to have first found entrance into Europe at the time of the overthrow of the Gothic monarchy in Spain by the Moors, when, to avenge the well-known outrage upon his daughter, "Count Julian called the invaders." Whensoever and wheresoever it came, it spread with fearful rapidity and havoc.

What is worthy of special remark is this :

that while almost all men are prone to take the disorder, large portions of the world have remained for centuries entirely free from it, until at length it was imported; and that it then infallibly diffused and established itself in those parts.

Of the more modern history of the disease our knowledge is more precise and sure. It tends uniformly to the same conclusion.

There was no small-pox in the New World before its discovery by Columbus in 1492. In 1517 the disease was imported into St. Domingo. Three years later, in one of the Spanish expeditions from Cuba to Mexico, a negro covered with the pustules of small-pox was landed on the Mexican coast. From him the disease spread with such desolation that within a very short time, according to Robertson, three millions and a half of people were destroyed in that kingdom alone. Small-pox was introduced into Iceland in 1707, when 16,000 persons

were carried off by its ravages—more than a fourth part of the whole population of the island. It reached Greenland still later, appearing there for the first time in 1733, and spreading so fatally as almost to depopulate the country.

Evidence to the same effect is furnished by the results of vaccination in some countries. To take one instance : vaccination was adopted in Denmark in 1801, and made compulsory in 1810. From that time small-pox disappeared altogether for fifteen years ; whereas, during the twelve years preceding the introduction of the preventive disorder, upwards of 3,000 persons died of the small-pox in Copenhagen alone.

Now it is a very instructive fact respecting this disease thus rankly contagious, and arising from no other source than contagion, that when it is epidemic in any place, many instances of it occur which we can by no means trace to contagion. The late Dr. Gregory declared that

of the numerous cases received into the Small-pox Hospital (to which he had long been physician) not one in twenty was capable of being referred to any known source of infection, the disease being ascribed by the patient to cold, fatigue, change of air, or some other innocent circumstance. About fifty years ago a prisoner, who had been for some time in solitary confinement in the Penitentiary at Millbank, was seized with small-pox. There neither was, nor had been recently, any other case of the disease within or near that building. Here, apparently, was an instance of small-pox occurring *de novo*, independently of contagion. The mysterious fact was mentioned in some lectures of mine published in 1843. In 1860 I received from Dr. Pratt, a retired physician who had been in practice at Kennington, a letter which solved the mystery. While on a visit to his son in Wales, who also was a medical practitioner, he there fell in with a copy of my lectures, and

read the remarkable case therein mentioned, and he wrote to me to explain it. Dr. Pratt's father had been the Resident Medical Officer at the Penitentiary, and was in the occasional habit of visiting his son at Kennington, who on one occasion took him to see, what is rare, a case of confluent small-pox in a man who had previously passed through the same disease. From his sick-room the father returned straight to the Penitentiary and to the prisoner's cell, who some days afterwards sickened of the disease. The resident officer was aware that he had been the vehicle of the contagion, but did not dare to confess it, so strict was the Committee of the Penitentiary in enforcing the rule that, on pain of expulsion, he should visit no sick person outside the walls of the prison.

Another case, of like significance, may be quoted from Dr. Gregory's book.

"In 1835 a child took small-pox in the country under circumstances which seemed to exclude all suspicion of

contagion. She had never left the house for several weeks, the few neighbours who had called were free from sickness, and no small-pox existed in the neighbourhood. During her convalescence, a looking-glass was put into her hands, when she said immediately, 'My face is exactly like that of the child at the door from whom I bought the beads.' On inquiry it was found that some pedlars had passed through the village, and that the child, though she had never left the house, had been to the door."

Dr. Gregory remarks that had this child died, or been an inattentive observer, the origin of this attack of small-pox must, on the principle of contagion, have for ever remained mysterious.

Of the features common to all these disorders the most astonishing is that of their non-recurrence in the same person. Rare exceptions to this rule are common also to them all. Small-pox has even been known to affect the same person thrice. A painful instance of its occurrence for the *second* time forces itself upon my recollection as I write. My friend, the Reverend Dr. Jelf, Canon of Christ Church, and formerly

Principal of King's College, London, died at Oxford of small-pox which he had contracted in Ireland. He had had the disease by *inoculation*, in his youth.

Various explanations of this remarkable quality have been attempted. The simplest and most plausible, I think, is that which appeals to the analogy of the corn-field. Farmers have learned, by long experience, that they cannot profitably crop ordinary ground with the same grain year after year. Some ingredient of the soil, probably some mineral matter, necessary for the germination of the seed, seems to be exhausted by repeated croppings, and it becomes requisite that the field should lie for a while fallow, or be replenished by manuring. It surely is conceivable that a soil may contain the needful ingredient in quantity so minute that a single crop may appropriate and exhaust it, and this without prejudice to the capacity of the soil in respect of other grain. In like

manner there may be, prior to the sowing of the virus, something analogous in the human body which a single crop of pustules removes entirely.

The exhausted soil of the corn-field recovers its lost power by lapse of time, and by treatment. Note here again the well-known fact that in the human system the protective influence against small-pox of distant bygone vaccination is apt to wane or wear out, presumably by a renewal, somehow, of the special ingredient; and thus the necessity for *re*-vaccination may arise.

Of chicken-pox little need be said. It is quite distinct from small-pox, and from that modified form of small-pox which not seldom is seen in persons partially protected by previous vaccination, and which has been mistaken for chicken-pox. It is a very trivial disorder, but it is stamped with the characters peculiar to the group. It is contagious, it runs a

definite course, it occurs but once in the same person.

Typhus fever is flagrantly contagious. Even in hospitals, where cleanliness and ventilation are prized and enforced, this fever attacks most of the persons who come oftenest and most closely in contact with those already sick of it : chiefly the nurses, next the clinical assistants, and the most assiduous of the students, and the medical officers ; and too frequently other patients in the same ward. "Every physician," writes Dr. Tweedie, whose testimony relates to typhus rather than to any other species of fever—

"every physician connected with the London Fever House, with one exception, has been attacked with fever, and three out of eight have died of it. Also the resident medical officers, matrons, porters, laundresses, domestic servants not connected with the wards, and every female who has performed the duties of nurse, have one and all invariably been the subjects of fever. And, to show that the disease is capable of being conveyed by fomites or clothes, the laundresses, whose duty it is to wash the

patients' clothes, are so invariably attacked with fever, that few women will undertake the loathsome and dangerous office."

I give one instructive history by way of sample. In the year 1866 a widow was living with one of her daughters in the small village of Scorton, in Lancashire. This was considered a model village ; it contained no public-house or beer-shop, and its inhabitants were quite healthy and free from fever. Another of the widow's daughters fell ill of typhus fever at Wigan. The daughter from Scorton went to Wigan to attend upon her sister, and was herself attacked. Shortly afterwards the mother went to their assistance, and she too caught the malady. The daughter who had first left her home died ; the other returned with her mother to Scorton, both of them being then in a convalescent state. On arriving at Scorton, they were helped out of the vehicle which took them from the railway station to their house by a man who soon afterwards was attacked with the fever, and died.

The disease then appeared in the house of a man named Wells, for whom the widow used to wash. Three of Wells' family were taken ill, and one of his daughters died of the disease. Gradually the epidemic spread from house to house, till of the thirty-six families living in the village twenty-eight were more or less affected with the fever. Eighty cases occurred in and around the village, and ten of them were fatal.

The contagious property of typhus fever is doubted now, I suppose, by none. The evidence of that of enteric fever requires a somewhat closer scrutiny. As we see this fever, we who live *in a city*, its contagious property is much less manifest than that of typhus. It is very rarely found to pass from one person to another in the wards of a well-conducted hospital, or in a private dwelling-house. Dr. Murchison tells us that during his own experience in the London Fever Hospital, of 1,048 cases of this enteric

fever there were but two which appeared to have originated in the hospital ; whereas, the number of typhus patients having been 2,581, thirty-six of the attendants and patients contracted the disease. Many of the continental physicians, and not a few in this country, doubt whether this fever be contagious at all. Dr. Murchison himself says that "although it is communicable, it is not so certain that it is contagious in the strict sense of the term."

On the other hand, in *rural districts*, in the hamlet and the farm, both here and abroad, the disease in numberless instances has been seen to spread from person to person, in the same house or neighbourhood, with a readiness scarcely less remarkable than that of typhus, so that the strange conclusion has been adopted that "enteric fever is almost invariably contagious in country places, and only rarely so, and by exception so to speak, in cities and large towns."

This apparent discrepancy requires and admits

of explanation. The whole subject has been handled with singular clearness and force by Dr. William Budd.

He enjoyed unusual and peculiar advantages for studying the country habits of this cruel disease. Born and bred up in the village of North Tawton, in Devonshire, he was personally acquainted with every one of its inhabitants ; and being in almost exclusive possession of the practice there, nearly every one who fell ill, not only in the village itself but over a large area around it, came immediately under his care. "For tracing," he says, "the part of personal intercourse in propagating disease, better outlook has rarely fallen to the lot of an observer."

He gives some striking examples of the spread of the fever through the household when once introduced.

In one family a young woman was the first subject of it. Her mother, brother, and sister were soon laid up with the same fever. The

father, who had already in former years had the disease, and a young infant, were the only inmates spared. In another house, four out of six persons were successively attacked ; in another, three ; and so on. There were few houses in which, having once appeared, it did not further extend itself to one or more members of the family.

All this might possibly have been the effect of some local miasm. Evidence much more cogent and conclusive of the contagiousness of a disease is its conveyance (by the transference of the sick person) to a distant spot, previously free from the malady, and its beginning at once to spread from that spot as from a centre ; for here we take account not simply of the extension of the malady to a few, but also of the previous freedom from it of the many.

Evidence of this kind, and in ample measure, is adduced by Dr. Budd in respect of enteric fever. Thus in North Tawton A. and B. lodged

next door to a house in which enteric fever existed. The two houses had a common privy. Both of these men took the disorder, and, becoming too ill to work, returned to their respective homes in the parish of Morchard, seven miles off. A. took at once to his bed, and at the end of five weeks was dead. Ten days afterwards his two children were laid up with the same fever, and had it severely. B., a single man, lodged with two aged persons. After a long struggle he recovered. When at his worst, a friend, C., who had called, was asked to assist in raising him in bed, and was overpowered by the smell from the sick man's body. Ten days afterwards, in another part of the village, C. was also "down" with the fever. Before he was convalescent, two of his children sickened with the disease, and also a brother who lived at a distance, but had repeatedly visited him.

The houses occupied by these four men lay some way apart. Except under their roofs,

there was at that time no fever in that part of the country.

Again, D., drooping with the disease, went from North Tawton to a house in Chaffcombe, seven miles distant. Nine persons in that house took the fever.

One of these nine moved in her early illness to Loosebeare, a hamlet four miles off, and became the focus of a little epidemic which gradually extended to the whole hamlet. Scattered over the country-side were some twenty or thirty other hamlets, the precise counterparts of this.

“Two or three farmyards, and a few labourers’ cottages clustered round them, made up (Dr. Budd says) in each case the little community. In each of these there were the usual manure yard and the inevitable pigsty ; in each there was the same primitive accommodation for human needs. The same sun shone upon all alike through month after month of the same fine dry autumnal weather. From the soil of all, human and other exuviae exhaled into the air the same compounds, in about equal abundance ; and yet, while at Loosebeare a large proportion of the inhabitants were lying prostrate with intestinal fever, in not

one of the twenty or thirty exactly similar places was there a single case."

In all cases the effluvia which proceed from the lungs and from the skin doubtless contain and help to carry the peculiar poison. But in enteric fever the contagious power resides chiefly and especially in certain discharges from the intestines.

I must here state that the distinctive anatomical character of this enteric fever is the occurrence, in a particular portion of the bowels, of a specific form of ulceration, which gives rise to a peculiar flux. It is believed and argued by Dr. Budd, and in the same belief I unhesitatingly share, that this peculiar flux from the intestines contains the infective poison of enteric fever, just as surely as the pustules on the skin in small-pox are pregnant with the specific contagium of that disease.

This being so, the apparent discrepancy between the town and country experience respect-

ing the contagious quality of this fever is accounted for at once.

In the country, under the present neglect of sanitary care, the poison of this disease, and of some others, *infects the ground*. The alvine discharges accumulate day by day on the open soil which surrounds the patient's dwelling until they envelope the whole household, and often the neighbours' also, in a fever miasm which is incomparably more virulent than the atmosphere of the sick chamber itself. Among the poor (I am still quoting from Dr. Budd), when this fever breaks out in a family, the discharges from the bowels are thrown either into the common privy, or, as I have seen a hundred times in rural districts, are cast upon the dung-heap or into the open gutter. From this point, following the line of watershed, this pestilent stuff often makes its way to considerable distances, where, appearing now under the guise of an endemic miasm, it may carry disease and

death into many an unsuspecting household. On the other hand, in country mansions, in cities, and in large towns, the alvine excretions—the principal vehicles of the poison—are for the most part discharged through water-closets into the drains and common sewers, and so cease to be dangerous *to the houses in which they are thrown off from the body of the sick person*, and the disease *seems* to be but slightly contagious.

But, according to Dr. Budd's teaching, the danger thus carried *from* the houses of the sick is transferred *to* other houses more or less distant. During the prevalence of enteric fever in a large town a vast quantity of the dejections peculiar to that disease must be daily and hourly poured into and through the drains and sewers, impregnating the sewer-gases with the specific poison. In our present faulty arrangements these gases, so infected, enter many a house, the inmates of which are unsuspecting of

such a source of fearful peril. Mr. Rawlinson, the well-known engineer, has stated in print that in the year 1859 disinfectants were freely used in some of the main sewers of London, and "the smell of the disenfectants was found to pervade all the houses in the district connected by drains with these sewers, showing to demonstration that such houses must, at all other times, be pervaded with diluted sewer-gases."

Wherever (writes Dr. Budd) the alvine discharges from enteric fever patients travel—wherever exhalations from them penetrate—there the most specific of all the exuviæ of the sick body are in operation. *The sewer, which is their common receptacle, is the direct continuation of the diseased intestine.*

Moreover, each of the two species of fever perpetuates itself, and no other. Typhus un-deviatingly reproduces typhus, and never enteric fever, any more than small-pox propagates scarlet fever or measles ; and so of the rest.

Upon this point we are indebted to Sir William Jenner for the most satisfactory evidence that has yet been collected. He noticed, as others have done, that whichever of the two species was, in its turn, prevalent in epidemic abundance, cases of the other were intermixed, and that these retained, without change or modification, their characteristic features. There were no transition-forms, nothing like graduation of one species into the other. He also took pains, during the years 1847, 1848, and 1849, to track the in-patients of the Fever Hospital to their several homes; and he found that where more than one fever patient came from the same family or the same house, they all presented the same species of fever. He thus traced 208 patients from 75 different sources of infection, with one exception only to the rule just stated; and this sole exception admitted of an explanation so reasonable that it could scarcely be said to militate against the general inference.

Some differences are noticeable in respect to the facility with which these diseases are caught. Thus the contagium of measles is rapidly diffusible through the air. Occurring in any one boy in a boarding-school, the disorder is sure speedily to affect all or nearly all the others who have not previously had it. In this it differs remarkably from scarlet fever, which, by careful isolation, may generally be restricted to the first sufferer from it in a school or family. The recurrence of the complaint after a primary attack happens more frequently with measles than with any of the others. The same thing has indeed been supposed and said respecting scarlet fever, but there are several rashes which in appearance strongly resemble and are apt to be mistaken for that of scarlet fever, though they are far from being identical with it.

It is essential to my purpose to show, not only that all these diseases are contagious, but more especially that they have now no other origin

than contagion. And here my real difficulty lies in the fact that I am painfully at issue with the highest recognised authority on these subjects in this country, Dr. Murchison, whose able and exhaustive treatise on fevers, founded as it is upon most ample and extensive personal experience, has secured for him the applause and gratitude of our whole profession.

Dr. Murchison admits, with very slight qualification, the contagiousness of these diseases, but he asserts their occasional, nay, their frequent generation *de novo*, independently of contagion.

Thus of typhus fever he says: "The poison is generated by the concentration of the exhalations from living beings, whose bodies and clothing are in a state of great filth." "Typhus is due to a specific poison." "The poison is also generated *de novo* by overcrowding and bad ventilation."

It seems to me that the last two quotations logically contradict each other.

The main argument for this view is the frequent occurrence of this disease and of its congeners when no exposure to contagion can be traced or easily imagined. I have already remarked upon the fallacious nature of this test when speaking of the production of small-pox.

The evidence, as Dr. William Budd has truly observed, is negative only, and consists solely in our inability to trace with the eye the continuity of a chain whose connecting links are known to be invisible. To conclude from this that no chain exists is palpably absurd.

There are a thousand unsuspected ways in which the invisible contagium may be conveyed. It may be distributed by the children of a day-school ; it may lurk in a hackney coach ; you may catch the complaint from your neighbour in an omnibus, at a theatre or a concert, at church, or in a casual jostling crowd ; your linen may be impregnated with it in the house of your laundress, or your coat may bring it from

your tailor's workshop; nay, I have heard it affirmed that the contagium of *small-pox* may be carried in a letter. These are not merely fancied dangers; they have been realized over and over again. One recent instance may here be cited by way of illustration of their kind. The Medical Officer of Health for Essex (Dr. Fox) reports this case, with many others like it:—

“Scarlet fever was exported from London into a public-house in Essex. While the children of the publican lay ill of this disease in the bedroom, their mother, who was in constant attendance upon them, descended to serve each customer who called to partake of refreshment.”

Again, we know that in instances innumerable, typhus fails to spring up where the effluvia constantly proceeding from the human body are accumulated and condensed by the crowding together of many persons in close, dirty, ill-ventilated places, where it could scarcely fail to appear if Dr. Murchison's theory were true. Dr. Bancroft instances the natives of the

Arctic regions, who, in order to shelter themselves against the extreme cold of their climate, live during the greater part of the year in close subterraneous dwellings, from which the fresh air is studiously excluded, and of which the atmosphere becomes so offensively foul as to be scarcely endurable by a stranger; yet typhus fever is not known among them. A similar exemption from that disease was observed within the tropics in the African slave-ships, where "the poor wretches were crowded together below the deck as close as they could possibly lie, in a sultry climate, barred down with iron to prevent insurrection. Although many of them died from suffocation and from fluxes, yet Dr. Trotter, who was himself at one time surgeon to a slave-ship, declares that "contagious fevers are not their diseases." Dr. Bancroft quotes also a narrative of the sufferings of 193 Europeans who, during the time of the first French Revolution, were "deported"

to Cayenne in the *Decade* frigate. They were crowded and even squeezed together in so small a space and for so long a time (no less than ninety-six days) that the sentinels who were placed at the hatchways to guard them, and who were thus exposed to the hot and foetid air which came from their hole of confinement, demanded that their period of this offensive duty might be shortened. But none of these miserable persons perished, nor did fever, properly so called, arise among them. Yet here were collected all the alleged causes of typhus fever, contagion excepted—crowding; want of ventilation, filthy clothing, unwholesome and corrupting food, anxiety and dejection of mind.

Typhus fever used to infest our English gaols, but that it was always imported, and never engendered there by filth and defective ventilation and by the accumulation of human effluvia, may be concluded from the fact that the bene-

volent John Howard, when he visited the prisons on the Continent, found to his great surprise that they were free from fever, although they were no less close, crowded, and impure than our own. He brings the outcome of his observations and inquiries concerning the gaol fever to this pointed conclusion:—

“ If it were asked what is the cause of the jail-fever, it would in general be readily replied, the want of fresh air and cleanliness ; but as I have found, in some prisons abroad, cells and dungeons as offensive and dirty as any I have observed in this country, where, however, this distemper was unknown, I am obliged to look out for some *additional cause* for its production—”

which additional cause can be no other than the contagious poison emanating from the bodies of those who have the fever. It is true that fever is most frequently met with, and most rapidly propagated, where men are crowded together in gaols or in close and ill-ventilated places ; but this affords no reason for supposing that it is ever *generated* there,

any more (to use a homely illustration of Dr. Bancroft's) than the prevalence of lice and other parasitic vermin in such places proves that these vermin are generated by filth, by pent-up human effluvia, and by want of ventilation, instead of being merely *fostered* thereby.

It was clearly shown by Dr. William Budd that the contagium of enteric fever could be fatally conveyed in the *water* used for drinking, and several very destructive outbreaks of the same malady have since been tracked to the consumption of *milk* contaminated by the specific poison. When we reflect how readily this origin of the disease may escape suspicion, or may elude detection even when suspicion has been roused, we find additional reason for distrusting the argument for the spontaneous development of the disease, based upon the difficulty of tracing the contagion to any conceivable source. For my own part I cannot but deem this argument altogether worthless.

Dr. Murchison holds that "the poison of enteric fever is contained in the emanations from certain forms of putrefying organic matter"—that it "is often generated by fæcal fermentation." Doubtless he would not insist on the ordinary meaning of the word "fermentation." His propositions, strictly expressed, signify that the poison of enteric fever is often generated during some part of the process of decomposition of human ordure : and he speaks of this as the *pythogenic* origin of the disease. But they who have had much experience of latrines in most parts of the Continent will be able to testify that the emanations from human excrement may be both intense and permanent without ever generating enteric fever. The same conclusion is warranted by the following remarkable statement made by a physician last year :

"According to the Corporation return for 1874, there are in one part of Edinburgh, congregated together and inhabited by the lowest of the population, no less than 14,319 houses, or dwellings—many under one roof, on the

'flat' system—in which there are no house-connections whatever with the street-sewers, and consequently no water-closets. To this day, therefore, all the excrementitious and other refuse of the inhabitants is collected in pails or pans, and remains in their midst, generally in a partitioned-off corner of the living room, until the next day, when it is taken down to the streets and emptied into the Corporation carts.

"Drunken and vicious though the population be, herded together like sheep, and with the filth collected and kept for twenty-four hours in their very midst, it is a remarkable fact that enteric fever and diphtheria, the two diseases that, in the words of Mr. Simon, are the 'direct emanations from the filthiest of all filth,' are simply unknown in these wretched hovels.

"Turning, however, to the fashionable or New town, where the houses are provided with all the modern conveniences, and communicate with drains which the natural contour of the city ought to render most effective, with here and there a cesspool, we find that enteric fever and diphtheria are never absent."

One convincing argument, to my mind, against the origin, sometimes, of these diseases from some other source than contagion, is the length of time which elapses in certain places without their occurrence at all. It is fit, therefore, that some authentic examples of long freedom

from them should be adduced, if they can be found.

Of long absences of *small-pox* from various places I have said enough already.

In the year 1846 an epidemic of *measles* spread itself through the group of small islands lying between Shetland and Iceland, and called the Feröe Islands. The disease was so serious and general that the Danish Government thought it necessary to send two physicians from Copenhagen, Dr. Manicus and Dr. Panum, to the relief of the islanders. Dr. Panum made a short but interesting report upon this epidemic in the *Archives Générales de Médecine* for April 1851.

In these islands, which are separated from each other by narrow and dangerous channels, and which are debarred from much intercourse with the world both by their geographical position and by their having no external commerce, measles had been totally unknown from

the year 1781. The disorder was brought to them in 1846 by a man who left Copenhagen on the 20th of March, arrived at the Island of Thorshavn apparently well on the 28th, and sickened on the 1st of April. In October the disease had again disappeared from the islands. During that interval of about six months, out of 7,782 inhabitants of the seventeen islands, 6,000 underwent the disease.

Notice here the entire absence of this complaint for sixty-five years, and its immediate and rapid diffusion upon the introduction of the contagion.

In our own island we see the measles chiefly among children and young persons. There it affected persons of every age. In a village containing one hundred dwellers, eighty were laid up with it at the same time.

All the old people who had had the complaint in the epidemic of 1781 escaped it in 1846.

This shows two things : First, that subsequent immunity from the disease is the rule. This rule was not broken in a single instance. Secondly, that the protection afforded by one attack does not wear out, in this disease, as life advances. The disorder proved very catching at the outset of the eruption and during its whole continuance. Isolation was the only sure defence against it.

Of the older persons living in 1781 who had not been exposed to the contagion (there were about 100 such), all took the disease in 1846. The explanation of the rarity of the disease in adults in this country is that a great majority of the whole population have had it during early life, and are therefore incapable of taking it later.

We read a similar lesson from the importation, some three years since, of measles into the Fiji Islands. Its instant, rapid, and wide diffusion, and its frightful mortality, may be taken as

proof that the disease had not for a long time, probably never, existed there before.

In 1863 I was informed by Dr. Anthoniz, who had practised for twenty years in Ceylon, that *scarlet fever* was unknown in that island. This accords with what I had for many years believed of the absence of scarlet fever from India. I had been assured by men of long and large experience of the diseases of India—by the late Sir Ranald Martin, by Dr. John Jackson, by Mr. Hewlett, who had the sanitary charge of the town of Bombay—that they had never seen or heard of that disease throughout our Indian dominions. Rumours, however, of its appearance there arose in 1871, and Surgeon Chapple of the Royal Artillery set the question at rest by his report of a series of cases of unequivocal scarlet fever which happened in the early part of that year at Kirkee. To Mr. Chapple also, after many years' service in India, the presence of the disease there was a novelty.

It was clearly imported from this country. Kirkee is a large artillery station within six hours by rail from Bombay. On the 31st of January, 75 artillerymen, 7 women, and 12 children landed at Bombay from the troop-ship *Euphrates*. Two days afterwards they were sent by rail to Kirkee. Several cases of scarlet fever had occurred on board the *Euphrates* during her voyage from England. On the 20th of February a child was admitted into the hospital at Kirkee with scarlet fever; on the 26th two more children; on the 27th another. Cases continued to occur up to the end of April, when the disease ceased to show itself.

Dr. Jackson told me in 1863 that *hooping-cough* had, till a short time before, been unknown at the Cape of Good Hope; but having been at length imported, it spread like wild-fire, so that a quarantine was there established against that disorder.

The late Sir James Simpson, to whose

writings I shall presently have occasion more particularly to refer, held the opinion which I have been taking some pains to justify. Speaking of small-pox, he says :

“We could no more expect this known species of disease or poison to originate *de novo* at the present day, under any combination of circumstances, than we could expect a known species of animal or plant, as a dog or a hawthorn, to spring up *de novo* without antecedent parentage.”

In illustration of the genesis of all the diseases now under consideration, I may adopt the playful language of a statesman, satirist, and wit of the last generation :

“Like genders like, potatoes ’tatoes breed,
Uncostly cabbage springs from cabbage seed—”

and from nothing else.

It might be, I fancy it has been, argued that, the causes assigned for the *de novo* origination of these diseases being in continual and widespread operation, the amount of *contagious* disease thus ever augmenting, thenceforward to

propagate its like, would in no very long time suffice to depopulate the world.

To the vitality, so to speak, of these contagia, it would be difficult to assign any limit of time. Of those which adhere to clothes and the like, as the contagium of small-pox or of scarlet fever, seclusion from the air would probably preserve indefinitely the infective power. We are here brought again upon the analogy of the corn-field, by the well-established fact that grains of wheat have germinated, and grown, and borne fruit, after having been imbedded and dormant for at least 3,000 years in the cerements of an Egyptian mummy.

Southey, in a note to vol. i. of his "Progress and Prospects of Society," states that in Dr. Franklin's works* an extraordinary circumstance is noticed as having occurred in London about the year 1763. Several medical men who assisted at the unfolding of a mummy died of a

* Vol. vi. p. 305.

malignant fever, which it was supposed they caught from the dried and spiced Egyptian.

Nine years ago Sir James Simpson put forth "A Proposal to stamp out Small-pox and the other Contagious Diseases." He stated that during the ten years from 1856 to 1865, small-pox destroyed in this island 51,034 persons. In 1864 the mortality reached to 9,425. He calculated that in the same decade of years, not less than 600,000 of the population of the United Kingdom had died of that formidable quaternion of diseases, small-pox, scarlet fever, measles, and hooping-cough.

Now, if my present contention be well founded—if, that is, all the diseases of our group are as surely due as small-pox is to contagion *only*—I may use the same arguments as he did in favour of his project for getting rid of the whole group.

The dreadful cattle-plague which invaded England in 1865, and has repeated the invasion

this very year (1877), is fruitful in instruction to us here. No other instance has been known to us of a contagious quality so intense, so far-reaching, so tenacious withal and abiding, so fatal, as that evinced by this murrain. Learned commentators have expressed their opinion that it is the same kind of pestilence as that which formed one of the plagues of Egypt. Yet vigorous measures resolutely carried into execution were successful in expelling it from among us, and doubtless will again succeed. The vast pecuniary loss inflicted by its presence was a sufficient motive for the most strenuous efforts to exterminate the scourge. Surely motives far higher and more powerful exist for rooting out our zymotic diseases. Similar measures are applicable to both cases. We cannot indeed slay the human subjects of zymotic disease and those suspected of it, but we may destroy the poison which they bear within and about them.

To this end the requisites are, first, the un-

failing and immediate notification to the proper authorities of the occurrence of every case. Second, the instant isolation of the sick person. Third, the thorough disinfection of his body, clothes, furniture, and place of isolation. Fourth, vigilant and effectual measures to prevent the importation of disease from abroad, and to strangle it should it by mischance return.

That such liberation from, and protection against, these diseases are feasible, I cannot doubt. The science of State Medicine—what the French call *hygiène publique*—is yet in its infancy in this country; but it has at length been *born*, and our Medical Council, and in harmony with it our Universities of Oxford and Cambridge, are nursing its growth by demanding from such of their graduates as may be ambitious of devoting themselves to the especial service of this new, noble, and hopeful science, full proofs of their competence to fulfil its peculiar claims.

Meanwhile the minds and writings of Mr. Simon, of the late Professor Parkes, of Dr. George Wilson, and of the staff of able men trained under the Local Government Board, may surely be trusted for devising and organising a machinery through the instrumentality of which the momentous exploit advocated in this paper may be effectually accomplished.

What a prodigious mass of premature deaths might in this manner be prevented, may be gathered from the records of the Registrar-General. These enumerate the *killed* alone. Far greater, and indeed innumerable, is the multitude of the wounded, the maimed, the disabled, the impoverished, by the stroke of these dread diseases, which thus bring widespread ruin and misery upon whole families at once.

It may possibly be objected that to enact the isolation, which might be stigmatised as the imprisonment, of those affected with these diseases

till they are incapable of imparting their disease to other persons, would be an unwarrantable infringement of the "liberty of the subject." But the objection will not bear examination. Our personal liberties must be, and daily are, restrained, when they would be in conflict with the general safety. The Legislature, for instance, does not scruple to enforce the isolation of a homicidal madman. Sir James Simpson puts the matter in a striking light.

"A rattlesnake or a tiger, escaped from a travelling menagerie into a school full of children, would in all probability not wound and kill as many of those children as would a boy or girl coming among them infected with, or still imperfectly recovered from, small-pox, or scarlet fever, or measles, or hooping-cough. Most properly, therefore, the cobra and the tiger, because they are *always* dangerous, are always as far as possible prohibited from making such visitation; and the infected boy or girl should be prohibited also *during the time* that they are dangerous, while they exhale from their bodies a virus of disastrous and deadly potency."

Nor does the economic aspect of the question require much consideration, though it is a scare-

crow to ratepayers. Upon them, and upon the commonwealth, the continuance of these disorders among our people would unquestionably levy annually a far heavier pecuniary tax and loss, than many multiples of the one cost of their extinction.

The abolition of zymotic disease, which our insular position would greatly favour and facilitate, is then "a consummation devoutly to be wished," but it cannot be looked for in the lifetime of an old man in his eighty-sixth year; yet he may not be too sanguine in trusting that it will be witnessed in the next generation, or at least by his grandchildren.

NOTE.—In the text of this paper I took pains to prove, by reasoning founded on acknowledged facts, that belief in the pythogenic origin of typhoid fever—that is to say, its occasional origin in the emanations generated during some part of the process of decomposition of human ordure—was a mistake.

I have since received fresh and striking confirmation of the soundness of that conclusion.

From an elaborate volume, published in 1878, on

“Typhoid Fever,” by Mr. William Thompson, it appears that the disease was *quite unknown* in Queensland, until it was brought there from England in the *Flying Cloud*, a fever-stricken emigrant ship. “It was taken to the Gulf Districts by the ship *Margaret and Mary*, and caused the deaths of many pioneers, stamping the north of Carpentaria as a dangerous fever district.”

Again : “Typhoid fever first appeared in Victoria in 1842. Three men from the immigrant ship *Salsette*, that had typhoid fever on board, were engaged as farm servants. Two of them died of it, and others about the farm were attacked by the fever in a similar way, proving its contagious nature.”

Here, then, are present all the materials and conditions for producing pythogenesis ; but typhoid fever does not occur till it has been imported. In other words, it proceeds from contagion only.

II.

HYDROPHOBIA AND RABIES.

HYDROPHOBIA AND RABIES.

IN the number of the *Nineteenth Century* Review for May, 1877, I contended, successfully I think, that the group of diseases rightly included among those called zymotic may, by means of wise legislation, and the equipment of suitable machinery, be eventually banished from this island. The favourable reception of my paper by many competent judges of its subject-matter encourages me to speak of another disease, also very destructive of human life, though numerically not *so* destructive as these, but even more dreadful and alarming to the mind than any of them. This plague also I

hold to be one of which we might get permanently rid. The disease, or rather the pair of diseases, to which I advert consists of hydrophobia in the human species and rabies in the canine. It is well to keep in mind the distinction between these two. There would be no hydrophobia were there no rabies; there can be no rabies unless it be communicated by a rabid animal; but they are not identical diseases. To use the concrete form of speech, rabies in the *dog* is quite different and distinct from hydrophobia in the *man*. The term hydrophobia is often erroneously applied to both diseases, but the rabid dog is never hydrophobic.

There has been an astonishing increase of hydrophobia in this country within the last half-century. Mr. Cæsar Hawkins, writing in 1844, says that only two cases of the disease had been admitted into St. George's Hospital since he first knew it twenty-five years earlier. Mention of such cases is constantly being made,

now, in the newspapers. Since the beginning of the present year no less than thirteen deaths from hydrophobia have been recorded within the limits of the London Registration.

So many erroneous notions are afloat on this subject that it may be neither uninteresting nor useless to the general reader to have a plain, untechnical history of the two diseases, which are inseparably connected by reciprocal relationship, the one being the parent of the other. In the canine race rabies can propagate rabies ; but hydrophobia does not (as I believe) ever reproduce itself.

The first thing to be noticed about hydrophobia is, that, frequent as it has become, many medical men pass through life without witnessing the disease at all. Hence there has, strangely enough, sprung up in some minds a fancy that no such disease has ever happened. Sir Isaac Pennington, who was in my time the Regius Professor of Physic at Cambridge, and

who had never seen a case of hydrophobia, could not be persuaded that any one else had seen anything more than a nervous disorder, produced by the alarmed imagination of persons who, having been bitten by a dog reputed to be mad, and having the fear of feather beds before their eyes, have been frightened into a belief that they were labouring under hydrophobia, and ultimately scared out of their very existence. It was at that time currently believed, at least by the vulgar, that any one afflicted with this terrible disorder was dangerous to those about him; and it was customary for his neighbours or associates to put an end at once to his woes and to their own cowardly dread of him, by smothering him between two feather beds.

But a far more eminent man than the Cambridge Professor, even Sir George Cornwall Lewis, was possessed with a similar incredulity on this subject, until convinced of his error by

Mr. Hawkins, who had then seen eleven or twelve cases of hydrophobia ; a larger number than perhaps any man in this country ever saw before or since. One reason for this was that he had received from Sir Robert Ker Porter, our Minister in South America, specimens of a substance called *guaco*, a supposed preventive and cure of hydrophobia and of snake-bites, and had on that account been summoned to cases of hydrophobia by various other practitioners.

I have myself seen four cases of that fearful malady, and I feel sure that no one who has even once watched its actual symptoms could fail to recognise it again, or could mistake any other malady for it, or wish to witness it thereafter. What these truly remarkable symptoms are I shall explain presently. It would, *a priori*, seem incredible that so many persons who have been bitten by mad dogs should have suffered so precisely the same train of symptoms, and

have at last died, from the mere force of a morbid imagination. But a single fact conclusive against such a belief is that the disease has befallen infants and idiots, who had never heard or understood a word about mad dogs or hydrophobia; and in whom the imagination could have had no share in producing their fatal distemper.

The steady increase in the population of this kingdom implies a corresponding, though perhaps not proportional, increase in the number of its dogs. In this way the area is ever growing larger of a field ready for the reception of the poisonous germ of rabies, and for the production in due time of a more or less copious crop of hydrophobia. The report for this year of the Postmaster-General contains the strange statement made by the local Postmaster of a large town in the North of England, that in the year 1876 twenty per cent. of his men—one in every five—were bitten by dogs. A Parliamentary

return of last session tells us that in the year ending with last May, 973 sheep and lambs were killed by dogs in ten of the counties of Scotland, and in most cases the owners of the dogs could not be discovered. There is in London a Home for stray and lost dogs. It has been affirmed in print by the well-known Secretary to the Society for Preventing Cruelty to Animals that upwards of 1500 dogs are taken to this Home every month. It is notorious that the tax on dogs is evaded to an enormous extent. All this serves to disclose the presence among us of a national nuisance, and a growing source of national dishonesty and of serious national peril. It is grievous to me to have to write in a strain so depreciatory of a race of animals that I love so well. But *corruptio optimi pessima*. It is an illustrative fact that, according to the Reports of the Registrar-General, no less than 334 persons died in England of hydrophobia in the decade of years ending with 1875.

Like other specific diseases springing from contagion, hydrophobia has its period of incubation ; and it is a somewhat variable period, lying for the most part between six weeks and three months. From a tabular account of 130 cases of the disease, referred to by Mr. Hawkins, it appears that five-sixths of the whole number occurred between eighteen days and three months. Mr. George Rigden, of Canterbury, has lately stated in the *Lancet* the following remarkable fact. He saw many years ago in one of the hospitals in London two patients who had been bitten at the same time by a cat which had been bitten by a rabid dog. Although the two patients had severally received their bites within a few minutes of each other, the respective outbreaks of hydrophobia were separated by an interval of two weeks. A like uncertainty of the access of the disease has been noticed among infected dogs. On the night of June 8, 1791, the man in charge of Lord Fitz-

william's kennel was much disturbed by fightings among the hounds, and got up several times to quiet them. On each occasion he found the same dog quarrelling ; at last, therefore, he shut that dog up by himself, and then there was no further disturbance. On the third day afterwards the quarrelsome hound was found to be unequivocally rabid, and on the fifth day he died. The whole pack were thereupon separately confined, and watched. Six of the dogs became subsequently mad, and at the following widely different intervals from the 8th of June, namely, 23 days, 56, 67, 81, 155, and 183 days.

Much longer periods, however, than any that I have hitherto mentioned are on record. In one instance, which was treated in Guy's Hospital, and the particulars of which were carefully investigated by Dr. (now Sir William) Gull, the disorder broke out more than five years after the patient had been bitten by a pointer-bitch

below his left knee. There a scar was visible, and the hydrophobic outbreak was preceded by pain in that spot. In the first volume of the *Lancet* the case is narrated by Mr. Hale Thompson of a lad who died hydrophobic seven years after a bite by a dog on his right hip, where there remained a cicatrix. For twenty-five months before his death this patient had been in close confinement in prison, and out of the way of dogs altogether.

Long periods of this kind cannot reasonably be regarded as periods of genuine or normal incubation. In explanation of them I some forty years ago published certain views of my own, but I do not know that they have been (to use a barbarous modern term) endorsed by any of my professional brethren. I imagine that the virus implanted by the rabid animal may remain lodged in the bitten spot, shut up perhaps in a nodule of lymph, or detained somehow in temporary and precarious union with some

one of the animal tissues, without entering the blood itself for a longer or shorter time—in some cases, perhaps, never entering it.* Some curious facts, fortifying this hypothesis of mine, have been noticed respecting another animal poison—the vaccine virus. The following statement is quoted by Mr. Grove, in the *Monthly Journal of Medical Science* for November, 1853:—

“A girl, aged fourteen years, was seized with influenza. She complained of pain in each arm at the spots where, when an infant, she had been vaccinated; and, in fact, in

* I find that Dr. Anthony Todd Thomson, in the thirteenth volume of the *Medico-Chirurgical Transactions*, 1826, has been tiresome enough to forestall me in this suggestion. He is commenting upon a case of hydrophobia caused by the bite of a cat, and he conjectures “that the virus remains dormant in the part where it is deposited by the tooth of the rabid animal, until a certain state of habit renders the nerves in its vicinity susceptible of its influence, and this being communicated, a morbid action is begun in these nerves, and extended to the respiratory nerves, which induce the whole train of symptoms constituting the disease.”

these places vaccine vesicles now became perfectly developed. An elder sister was revaccinated with lymph thence obtained; beautiful vesicles formed, and ran a natural course."

At the Obstetrical Society of London in 1860, Dr. Hodges stated that

in May, 1854, he vaccinated a little boy three years of age, but the arm did not "rise" within the usual period. In the following May, however, a vesicle spontaneously formed, with an areola on the seventh and eighth days, gradually declining on the eleventh and twelfth; a permanent cicatrix, marked by pits, remaining and giving evidence of the genuine vaccine disease.

If my hypothesis be well founded, it may account for some of the cases in which persons bitten by a rabid dog escape hydrophobia altogether.

The well-known fact that the bitten spot, wound, or scar very often becomes the seat of some fresh morbid phenomena (variously spoken of as pain, redness, swelling, coldness, stiffness, numbness, tingling, itching), which spread towards the trunk of the body just before the

paroxysmal symptoms of hydrophobia show themselves, is strongly in favour of the belief that the poison may lie inert in the place of the original hurt for some time, and then, in some obscure way, get liberated and set afloat in the circulating blood.

Pain, sensations of pricking, and other peculiar feelings, preceded the manifestation of the hydrophobic condition in three of the four cases seen by myself; in the fourth case no inquiries appear to have been made on that point. In another instance which Mr. Herbert Mayo witnessed, and the victim of which he examined after death, he found the inner part of the cicatrix blood-shotten, and a gland in the arm-pit had swelled at the coming on of hydrophobic symptoms; and I find among my notes of Mr. Abernethy's lectures another striking case still more to the purpose. A very intelligent boy had been bitten in the finger by a dog. He was taken into St. Bartholomew's Hospital. Caustic

had been freely used, affecting the sinewy parts, and producing a terrible sore ; yet the boy was recovering himself, and the sore was healing. One day, as Mr. Abernethy was going round the hospital, he saw and spoke to the boy, who said he thought he was getting well, but that he had on that day an odd sensation in his finger, stretching upwards into his hand and arm. Going up the arm were two red lines like inflamed absorbents. Doubtless they were such. Mr. Abernethy made light of the matter, ordered a poultice and some medicine. Early the next morning he again visited the ward, pretending that he had some other patient there whom he wished particularly to see ; and when going out again he asked the boy, in a careless tone, how he was. The boy said he had lost the pain, but felt very unwell, and had not slept all night. Mr. Abernethy felt his pulse, told him he was rather feverish, as might be expected, and asked him if he was not thirsty, and would like some

toast and water. The boy said he *was* thirsty, and that he *should* like some drink. When, however, the cup was brought he pushed it from him ; he could not drink. In forty-eight hours he was dead.

The symptoms of hydrophobia, stated in broad outline, are these. Excessive nervous irritability and terror, spasmodic contractions of the muscles of the throat, excited by various external influences, and especially by the sight or sound of liquids, and by attempts to swallow them, and sometimes absolute impossibility of swallowing them, earnest attempts to do so notwithstanding.

When fluids are offered to and pressed upon the patient, he will take the vessel containing them into his hand, but draws back his head to a distance from it with a repelling and apparently involuntary gesture ; meanwhile he makes a succession of hurried gasping sighs and sobs, precisely resembling those which occur when

one wades gradually and deeply into cold water. The sound of water poured from one vessel into another, gusts of air passing over his face, the sudden access of light, the waving of a mirror before his eyes, the crawling of an insect over his skin—these are things which in a hydrophobic patient suffice to produce great agitation, and the peculiar strangling sensation about the fauces. He goes on rapidly from bad to worse ; in most cases more or less of mania or delirium is mixed up with the irritability. Illusions of the senses of sight and of hearing are not uncommon. The sufferer is very garrulous and excited. In some cases, but not in all, there is incontinence of urine. Foam and sticky mucus gather in his throat and mouth, and he makes great efforts by pulling it with his fingers, and by spitting, blowing, and hawking, to get rid of it ; and the sounds he thus makes have been exaggerated by ignorance and credulity into the foaming and barking of a dog. In the same

spirit the palsy of his lower limbs, which sometimes takes place, rendering him unable to stand upright, has been misconstrued into a desire on his part to go on all fours like a dog. Vomiting is a frequent symptom. The pulse in a short time becomes frequent and feeble, and the general strength declines with great rapidity. Death occasionally ensues within twenty-four hours after the beginning of the specific symptoms. Most commonly of all, it happens on the second or third day; now and then it is postponed to the fifth day; and in still rarer instances it may not occur till the seventh, eighth, or ninth day.

Usually the paroxysms, becoming more violent and frequent, exhaust the patient; but occasionally the symptoms undergo a marked alteration before death. The paroxysms cease, the nervous irritability disappears, the patient is able to eat and drink and converse with ease, those sights and sounds which so annoyed and

distressed him before no longer cause him any disquiet. The late Dr. Latham had a hydrophobic patient under his care in the Middlesex Hospital. On going one day to the ward he fully expected to hear that the patient was dead, but he found him sitting up in his bed quite calm and free from spasm. He had just drunk a large jug of porter. "Lawk, sir!" said a nurse that stood by, "what a wonderful cure!" The man himself seemed surprised at the change; but *he had no pulse*; his skin was as cold as marble. In half an hour he sank back and expired.

It has been alleged that tetanus may be mistaken for hydrophobia, but the differences between the two are very clearly marked. It is true that slight touches of the body will excite the tetanic spasm, but it is the rigid or abiding form of spasm, which relaxes gradually and slowly; whereas in hydrophobia the spasms are sudden and frequent, such as are popularly

called convulsions. In tetanus there is no thirst, seldom any vomiting, no accumulation of tough and stringy mucus in the mouth and throat. The mental faculties are clear, and the patient is serene, and what is called heart-whole, to the last.

The symptoms of rabies, as witnessed in the dog, have been well described by Mr. Youatt. The earliest is a marked change in the animal's habits. Of course this will be more perceptible by those acquainted with the dog, and cognizant of his habits. The dog becomes sullen, restless, his eyes glisten, there is often slight squinting, and some twitching of the face, with a continual shifting of posture, a steadfast gaze expressive of suspicion, an earnest licking of some part on which a scar may generally be found. If the ear be the affected part, the dog is incessantly and violently scratching it ; if the foot, he knaws it till the skin is broken. Occa-

sional vomiting and a depraved appetite are also early noticeable. The dog will pick up and swallow bits of thread or silk from the carpet, hair, straw, and even dung. Then the animal becomes irascible, flies fiercely at strangers, is impatient of correction, which he receives in sullen silence, seizes the whip or stick, quarrels with his own companions, eagerly hunts and worries the cats, demolishes his bed, and if chained up makes violent efforts to escape, tearing his kennel to pieces with his teeth. If at large he usually attacks such dogs as come in his way, but if he be naturally ferocious he will diligently and perseveringly seek his enemy. About the second day a considerable flow of saliva begins, but this does not long continue, and it is succeeded by insatiable thirst. He appears to be annoyed by some viscid matter in his throat, and in the most eager and extraordinary manner he works with his paws at the corners of his mouth to remove

it, and while thus employed frequently loses his balance and rolls over. A loss of power over the voluntary muscles is next observed. It begins with the lower jaw, which hangs down, and the mouth is partially open ; the tongue is less affected ; the dog is able to use it in the act of lapping, but the mouth is not sufficiently closed to retain the water ; therefore, while he hangs over the vessel eagerly lapping for several minutes, its contents are very little, or not at all diminished. The palsy often affects the loins and extremities also ; the animal staggers about and frequently falls. Previously to this he is in almost incessant motion. Mr. Youatt fancies the dog is subject to what we call spectral illusions. He starts up and gazes earnestly at some real or imaginary object. He appears to be tracing the path of something floating around him, or he fixes his eyes intently on some spot on the wall, and suddenly plunges at it ; then his eyes close, and his head droops.

Frequently, with his head erect, the dog utters a short and very peculiar howl ; or if he barks it is in a hoarse, inward sound, totally unlike his usual tone, terminating generally with this characteristic howl. The respiration is always affected ; often the breathing is very laborious ; and the *inspiration* is attended with a singular grating, choking noise. On the fourth, fifth, or sixth day of the disease he dies, occasionally in slight convulsions, but oftener without a struggle.

It is a common and misleading mistake to think that the rabid dog, like the hydrophobic man, will shun water, and that if he takes to a river it may safely be concluded that he is not mad. On the contrary, as I have already hinted, there is no dread of water, but unquenchable thirst ; the animal rushes eagerly to water, plunges his muzzle into it, and tries to drink, but often is unable to swallow from paralysis of his lower jaw, which prevents him from shutting his mouth.

Another opinion not at all uncommon is that healthy dogs recognize one that is mad, and fear him, and run away from his presence, in obedience to some mysterious and wonderful instinct, warning them of their danger. According to Mr. Youatt this is quite unfounded. Equally mistaken is the notion that the mad dog exhales a peculiar and offensive smell.

I do not know whether the period of incubation in a dog which has been infected with rabies by the bite of another rabid dog has been accurately ascertained ; but that the disease may be imparted by a dog so infected before the symptoms of rabies become manifest is clear from the following instance, with which I have been favoured by Mr. Wrench, of Baslow, in Derbyshire :—

“A small terrier (he writes) belonging to myself was bitten by an undoubtedly rabid dog, and was consequently destroyed about a fortnight afterwards, and before it had shown any symptoms of disease. In the meantime it had licked the cropped ears of a bull-dog

puppy which had not been near the first-named rabid dog, and this puppy went mad about eight weeks after his ears were licked."

From what animals may the infection be received? We are sure that the disease, by the inoculation of which hydrophobia may be caused in man, is common in the dog; and that it has often been communicated to the human animal by the fox also, the wolf, the jackal, and the cat. The death from hydrophobia of a boy after being bitten by a racoon is recorded by Dr. Russell, of Lincoln, Massachusetts, in the *Transactions of the American Medical Association* for 1856. Mr. Youatt declares that the saliva of the badger, the horse, the human being, has undoubtedly produced hydrophobia; and some affirm that it has been propagated even by the turkey and the hen. The same author mentions a case in which a groom became affected with hydrophobia through a scratch which he received from the tooth of a rabid

horse. This would seem to settle the question as respects that animal ; but as horses, cows, and fowls do not usually bite, we have not many opportunities of furnishing a positive answer to the general question.

The grandfather of the present Duke of Richmond died, in Canada, of hydrophobia, communicated, it was then thought, by a fox. But I was told in 1862, on the authority of a person who was living at Montreal at the time of the Duke's death, and was acquainted with his family, that his disease was caused by the bite of a dog ; and I was afterwards informed by Mr. Lawrence Peel, the Duke's son-in-law, that it was uncertain whether the bite was made by a fox or by a dog. The Duke was interfering in a fray between a tame fox and a pet dog—the fox retreating into his kennel. It is not certainly known which of the animals had rabies.

The disease is said to have been caused by the *scratch* of a cat. Now we know that cats, as

well as dogs, frequently apply their paws to their mouths, especially when the latter part is uneasy, as it clearly is in mad dogs. The fact, therefore, of the production of the disease by a scratch from the claws of a cat, if thoroughly made out, would afford no proof, nor scarcely even a presumption, that the disease can be introduced into the animal system in any other way than by means of the saliva.

Several important questions at once present themselves respecting these two diseases.

First, is a man who has been bitten by a mad dog, and in whose case no preventive measures have been taken, a doomed man? I have answered this question in the negative already. Few, upon the whole, who are so bitten become affected with hydrophobia. John Hunter states that he knew an instance in which, of 21 persons bitten, only one fell a victim to the disease. Dr. Hamilton estimated the proportion to be 1 in

25. But I fear these computations are much too low. In 1780 a mad dog in the neighbourhood of Senlis took his course within a small circle, and bit 15 persons before he was killed ; three of these died of hydrophobia. The saliva of a rabid wolf would seem to be highly virulent and effective. These beasts fly always, I believe, at a naked part. Hence, probably, the fatality of their bites. The following statement relates exclusively to the wolf. In December 1774, 20 persons were bitten in the neighbourhood of Troyes ; 9 of them died. Of 17 persons similarly bitten in 1784 near Brive, 10 died of hydrophobia. In May 1817, 23 persons were bitten and 14 perished. Four died of 11 that were bitten near Dijon ; and 18 of 24 bitten near Rochelle. At Bar-sur-Ornain 19 were bitten, of whom 12 died within two months. Here we have 114 persons bitten by rabid wolves, and among them no fewer than 67 victims to hydrophobia ; considerably more than

one-half. There is no doubt, however, that the majority of persons who are bitten by a mad *dog* escape the disease. This may be partly owing to an inherent inaptitude for accepting it. There are some upon whom the contagion of small-pox has no influence. This peculiarity exists apparently even among dogs. There was one dog, at Charenton, that did not become rabid after being bitten by a rabid dog ; and it was so managed that at different times he was bitten by thirty mad dogs, but he outlived it all. Much will depend also upon the circumstances of the bite, and the way in which it is inflicted. If it be made through clothes, and especially through thick woollen garments, or through leather, the saliva may be wiped clean away from the tooth before it reaches the flesh. In the fifth volume of the *Edinburgh Medical and Surgical Journal* there is a case described by Mr. Oldknow, of Nottingham, in which a man was bitten in three different places by the same

mad dog, namely, in the groin, the thigh, and the left hand ; the bite on the hand was the last. Now it seems that but for this last bite, on a naked part, he might have escaped. It is noteworthy that the local sensations preliminary to the fatal outbreak of hydrophobia occurred only in the hand and arm. The attacking dog probably shuts his mouth after each bite, and thus recharges his fangs with the poisonous material. In a report from America it is stated that of 75 cases the injury was received on the hand in 40 instances, on the face in 15, on the leg in 11, on the arm in 9.

It is this frequent immunity from the disease in persons who have been bitten that has tended to confer reputation upon so many vaunted methods of prevention. Ignorant men and knavish men have not failed to take advantage of this. They announce that they are in possession of some secret remedy which will prevent the virus from taking effect ; they persuade the

friends of those who die that the remedy was not rightly employed, or not resorted to sufficiently early; and they persuade those who escape that they escaped by virtue of the preventive remedy. If the plunder they reap from the foolish and the frightened were all, this would be of less consequence; but unfortunately, the hope of security without their undergoing a painful operation leads many to neglect the only trustworthy mode of obtaining safety.

A still more anxious inquiry next arises. Whoever has been bitten by a rabid or by a suspected animal must be considered, and will generally consider himself, as being in more or less danger of hydrophobia. This dread is not entirely removed even by the adoption of the best means of prevention. Now, how long does this state of hazard continue? When is the peril fairly over? After what lapse of time may the person who has sustained the injury lay aside all apprehension of the disease? To this

inquiry no satisfactory reply can be given. In a vast majority of instances, indeed, the disorder has broken out *within two months* from the infliction of the bite. But the exceptions to this rule are too numerous to permit us to put firm trust in the immunity foreshadowed by that interval. Cases are recorded in which five, six, eleven, nineteen months have intervened between the insertion of the poison and the eruption of the consequent malady. Nay, there are well-authenticated instances, as I have already said, of the lapse of twenty-five months, of more than five years, or even of seven years. In these cases it is most probable that some unsuspected re-inoculation, some fresh application of the peculiar virus, has taken place. If not, then we must conclude that the poison really lies imprisoned in the bitten part, and only becomes destructive when, under certain obscure conditions, and at indefinite periods, it gets into the circulation.

I say nothing about the morbid appearances found in persons dead of hydrophobia, for they are not distinctly indicative of that disease,* and I am not addressing professional readers. But, as a help towards determining whether a dog which may have been destroyed under equivocal circumstances was indeed rabid, it may be useful to state that in the stomach of a really mad dog there are always to be found very unnatural contents—straw, hay, coal, sticks, horse-dung, earth—as well as a quantity of a dark fluid like thin treacle, altered blood in fact.

And here it may be well to deprecate and denounce a practice much too common with us,

* This statement I trust I may now venture to correct. Dr. Gowers informs me, that in three instances, at least, he has detected by microscopic research such physical changes in the medulla oblongata after death by hydrophobia as suffice to explain the symptoms of that disease; changes so peculiar as to enable him in future to pronounce that a person whose brain presented them had died of that disease.

that, namely, of at once destroying a suspected dog by which some one has been bitten, but about the true condition of which there exists no absolute certainty. The dog should be securely isolated and watched; a day or two will be sufficient for solving the anxious question. If he should prove really mad he should then of course be put to death, as mercifully as may be. If, on the other hand, he remains well, not only will the life of a possibly useful and favourite animal be saved, but, what is of incomparably greater importance, the mind of the bitten person will be freed from a harassing sense of dread, with which it might otherwise be haunted for years to come.

The most important question of all in relation to my present purpose, is whether rabies can be excited by any other cause than inoculation of the specific virus; in other words, whether it has any other source than contagion.

Many persons believe that the disease may,

and does often, arise *de novo* ; and causes have been assigned which certainly are not true causes. Thus it has been ascribed to extreme heat of the weather. It is thought by many to be especially likely to occur during the dog-days ; and to be in itself a sort of dog-lunacy, having the same relationship to Sirius that human insanity has to the Moon—which in one sense is probable enough. But abundant statistical evidence has been collected in this and in other countries, that the disease occurs at all seasons of the year indifferently. The cautions, therefore, which are annually put forth in hot weather, as to muzzling dogs and so forth, whatever may be their value, would be as opportune at any other time. The disorder has been attributed to want of water in hot weather, and sometimes to want of food, but MM. Dupuytren, Breschet, and Majendie in France, caused both dogs and cats to die of hunger and thirst, without producing the smallest approach to a state

of rabies. At the Veterinary School at Alfort three dogs were subjected to some very cruel but decisive experiments. It was during the heat of summer, and they were all chained in the full blaze of the sun. To one salted meat was given ; to the second water only ; and to the third neither food nor drink. They all died, but none of them became rabid. Nor does the suspicion that the disorder may have some connexion with the rutting period in these animals appear to rest on any better foundation.

Some, very interesting points still remain to be considered as to the communication of these diseases from one person or animal to another.

Mr. Youatt, whose experience on this subject was very large, did not think that the saliva of a rabid animal could communicate the disorder through the unbroken cuticle. He believed that there must be some abrasion or breach of surface. He held, however, that it might be communicated by the mere contact of the saliva with

the mucous membranes. Of its harmlessness on the sound skin he offered this presumptive evidence—that his own hands had many times been covered with the saliva of the mad dog with perfect impunity. He has recorded some singular instances in which hydrophobia and rabies were caused by contact of the morbid saliva with the mucous membranes. A man endeavoured to untie by the help of his teeth a knot that had been firmly drawn in a cord. Eight weeks afterwards he died undeniably hydrophobic. It was then recollected that with this cord a mad dog had been tied up. A woman was attacked by a rabid dog, and escaped with some rents in her gown. In the act of mending it she thoughtlessly pressed down the seam with her teeth. She also died. Horses are said to have died mad after eating straw upon which rabid pigs had died. Portal was assured that two dogs which had licked the mouth of another dog that was rabid were

attacked with rabies seven or eight days afterwards. Mr. Gilman, of Highgate, in a little pamphlet on Hydrophobia, quotes an instance from Dr. Perceval, in which a mad dog licked the face of a sleeping man, near his mouth, and the man died of hydrophobia, although the strictest search failed to discover the smallest scratch or abrasion on any part of his skin. These facts, if authentic, settle the question; unless, indeed the lips of those who perished happened to have been chapped or abraded.

It is a fearful question whether the saliva of a human being afflicted with hydrophobia is capable of inoculating another human being with the same disease. Mr. Youatt says it is, that the disease has undoubtedly been so produced. If this be so, the fact should teach us—not to desert or neglect these unhappy patients, still less to murder them by smothering, or by bleeding them to death—but to minister to their wants with certain precautions; so as not to

suffer their saliva to come in contact with any sore or abraded surface, nor, if it can be avoided, with any mucous surface. On the other hand, all carefulness of that kind will be superfluous if the disease cannot be propagated by the human saliva. Certainly many experimenters have tried in vain to inoculate dogs with the spittle of a hydrophobic man ; but there is one authentic experiment on record which makes it too probable that the disease, though seldom or with difficulty communicated, may yet be communicable. The experiment is said to have been made by MM. Majendie and Breschet, at the Hôtel Dieu, in Paris, and to have been witnessed by a great number of medical men and students. Two healthy dogs were inoculated on the 19th of June, 1813, with the saliva of a hydrophobic patient named Surlu, who died the same day in the hospital. One of these dogs became mad on the 27th of the following month. They caused this dog to bite others, which in

their turn became rabid also ; and in this way the malady was propagated among dogs during the whole summer. Now this, though a very striking statement, ought not to be considered conclusive ; for it is possible that the disease in the first dog might have had some unknown and unsuspected origin. We have enough, however, in this one experiment to make us observe all requisite caution when engaged in attending upon a hydrophobic patient.

In an elaborate and valuable treatise on "Rabies and Hydrophobia," Mr. George Fleming adduces conflicting evidence as to the safety or danger of drinking the milk of a rabid animal, and he wisely advises the avoidance of such milk. Pertinent to this question I have received from Mr. Wrench, of Baslow, even while this paper is passing through the press, the following history, which shows that the disease is transmissible from the mother to her offspring through the medium of her milk :—

“In the middle of May 1876, on Mr. Twigg’s farm, Harewood Grange, near Chatsworth, a mad dog bit eighteen sheep out of a flock of twenty-one, which were at the time suckling thirty lambs. The sheep were all bitten about the *face*, and had evidently been defending their lambs during the greater part of the night in which the attack was made. Mr. Twigg examined both sheep and lambs, and could not find a single wound on any of the latter. In about a month both sheep and *lambs* began to die at the rate of two or three a day. The sheep ran wildly about, sometimes carrying stones in their mouths, and the lambs ran away. Of the eighteen sheep that had been bitten sixteen died; and of the thirty lambs, not one of which was believed to have been bitten, fourteen died. On the next farm the same thing happened to a smaller extent.”

What can be said of the treatment of hydrophobia or of rabies? There is no authentic case on record that I am aware of in which a hydrophobic person has recovered. As it has been so it is still. Ἱατρὸς ἁλῶται θάνατος—the Physician that cures is Death. It would be idle to discuss any curative measures after the peculiar symptoms of the disease have once set in.

Not so, however, with respect to prevention ; that is the most important object of our practice—that and the euthanasia.

The early and complete excision of the bitten part is the only means of prevention in which much confidence can be placed ; and even that is open to a source of fallacy. In the majority of cases no hydrophobia would ensue, though nothing at all were done to the wound. No doubt many persons undergo the operation needlessly. But in no given case can we be sure of this. If excision should for any reason be impossible, the wound should be cauterised. Of the efficacy of the latter plan we have this evidence :—Mr. Youatt, who trusted to it, and who had himself been bitten seven times, tells us that he had operated with the lunar caustic—nitrate of silver—on more than 400 persons, all bitten by dogs unquestionably rabid, and that he had not lost a case. One man died of fright, but not one of hydrophobia. Moreover, a

surgeon of St. George's Hospital told him that ten times that number had undergone the operation of excision there, after being bitten by dogs (all of which might not, however, have been rabid), and it was not known that there had been a single fatal issue. Excision, in my judgment, must, when practicable, be the most trustworthy and eligible procedure. Trousseau recommends, as a ready and quick preventive, the actual cautery—that is, the destruction of the poison and the tissues of the bitten part by searing them with a red-hot iron. They might be as readily and thoroughly destroyed by brushing the interior of the wound, by means of a glass brush, with nitric acid.

But if the wound be of such a size and in such a place that it can be excised, what is the best method for its excision? This is the advice of my old master, Abernethy :—

“The cell (he says) into which a penetrating tooth has gone must be cut out. Let a wooden skewer be shaped as nearly as may be into the form of the tooth, and then

be placed into the cavity made by the tooth, and next let the skewer and the whole cell containing it be removed together by an elliptical incision. We may examine the removed cell to see if every portion with which the tooth might have had contact has been taken away: the cell may even be filled with quicksilver to see if a globule will escape. The efficient performance of the excision does not depend upon the extent, but upon the accuracy of the operation."

Early excision, then, is almost a sure preventive; but in all suspicious cases, if the operation has been omitted in the first instance, it will be advisable to cut out the wound or its scar within the first two months, or at any time before preliminary feelings in the spot foreshow the coming outbreak. Later would be too late. Dr. Richard Bright has recorded a case in which the arm was amputated, in Guy's Hospital, upon the supervention of tingling and other symptoms in the hand on which the patient had been bitten some time before; but the amputation did not save him.

The new power which we have happily ob-

tained of suspending sensation *generally* by the inspiration of certain vapours, or *locally* by the æther spray, will contribute at least to the prevention of hydrophobia by divesting the process of excision or cauterisation of its pain, and therefore of its terrors.

For my own part, if I had received a bite from a decidedly rabid animal upon my arm or leg, and the bite was such that the whole wound could not be cut out or thoroughly cauterised, my reason would teach me to desire, and I hope I should have fortitude enough to endure, amputation of the limb above the place of the injury.

As to the euthanasia, it may best be promoted by some narcotic drug ; and I know of none more eligible than the chloral hydrate, administered in such doses and at such intervals as may suffice, without shortening life, to quiet the restless agitation, and to mitigate the sufferings, of its inevitable close. Should the

patient be unable to swallow that remedy, recourse may be had, under similar limitation, to its subcutaneous injection, or to some anæsthetic vapour.

What, it may be asked, should be done by or for a man who has been bitten by a rabid animal, and has no access to immediate medical help? Should he, the wound being within reach of his lips, or should another person for him, try to suck out the inserted venom? That would probably be his first instinctive thought. But when I call to mind what Mr. Youatt has said of the danger attending the contact of the poisonous saliva with even sound mucous membranes—and further, the risk that the sucker's lips might, whether he knew it or not, be chapped or abraded—I dare not counsel the expedient of suction. By adopting it the sufferer might be rushing, or bringing his helping neighbour, into the very peril he was anxious to avert.

A cupping-glass would be a safer application of the same principle, provided that the place and size of the wound would admit of its being covered by the glass. But, at best, a cupping-glass extemporised and clumsily used under urgent and agitating circumstances, can scarcely be advisable.

What I should most strongly recommend, and fortunately it is very easy of performance, is this. First, that a bandage tight enough to restrain the venous circulation should be applied just above the wound, between it and the heart; and next, that without any delay a continuous stream of tepid or cold water should be poured from a height, and therefore with a certain degree of force, upon and into the wound. This might be done from the spout of a tea-kettle, or better from a water-tap, and it should be persevered with even for an hour or two, or until the arrival of medical aid. In this way the implanted poison would,

in all likelihood, be thoroughly washed away, and the safety of the sufferer secured. Nevertheless this process need not exclude subsequent excision or cauterisation, should one or the other be feasible or thought desirable, "to make assurance doubly sure."

The opinion which, as my readers must have anticipated, I entertain, that rabies has at present no other source than contagion, has been combated with the same arguments as have been used in the analogous case of small-pox ; such as that the disease must at some time have had a beginning, and therefore why not now ? that it often springs up where no contagion can be traced, and sometimes where contagion seems to be impossible. These arguments were discussed in my former paper, and their futility fully demonstrated. I refrain, therefore, from reconsidering them here : but as I then related two striking instances in which contagion had been deemed impossible,

but in which its operation was at length detected by some very singular evidence, so I will here give a condensed account of a like result under similar circumstances in respect of rabies.

Mr. Blaine, Mr. Youatt's partner, was consulted about a gentleman's dog, and pronounced it undoubtedly rabid. But the dog, it was alleged, had never for many months been out of doors, nor indeed out of the sight of its master, or, in the master's absence, of his valet, who had especial charge of the dog. Concurring with Mr. Youatt in opinion, and anxious to learn the truth in a matter so important, Mr. Blaine examined the servants very closely; and it was at length remembered by the footman that he had had to answer his master's bell one morning when the valet, whose business it was to take the dog from the bedroom, was accidentally absent; and he also distinctly recollected that the dog accompanied him to the

street door while he was receiving a message, went into the street, and was there suddenly attacked by another dog that was passing, seemingly without an owner. The wandering dog was, no doubt, rabid.

Again, a Newfoundland dog, which was chained constantly to his kennel during the day, and suffered to be at large during the night within an enclosed yard, became rabid ; and as no dog was known to have had access to the yard, the owner felt sure that the disease must have arisen spontaneously. Mr. Blaine, however, elicited the facts that the gardener to the family remembered to have heard when in bed one night an unusual noise, as if the Newfoundland dog was quarrelling with another. He recollected, also, that about the same time he saw marks of a dog's feet in his garden, which lay on the other side of the yard, and the remains of hair were noticed on the top of the wall. About the same time the neigh-

bourhood had been alarmed by the absence of a large dog belonging to one of the inhabitants, which had escaped from confinement during the night under evident symptoms of disease. Here also was a ready solution of the previous mystery.

I can pretend to no originality on this subject. Mr. Youatt believed that rabies in the dog and in all creatures results always from the introduction of a specific virus into the system. He maintained that a well-enforced quarantine for seven months—every dog in the kingdom being confined separately—would extirpate the disease. And the late Sir James Bardsley proposed a plan which he thought would prove efficacious for getting rid of this dreadful bane.

“It consists (he wrote) merely in establishing a universal quarantine for dogs within the kingdom, and a total prohibition of the importation of those animals during the existence of this quarantine. The efficacy of his preventive scheme rests upon the validity of the

following propositions. First, that the disease always originates in the canine species ; secondly, that it never arises in them spontaneously ; thirdly, that the contagion, when received by them, never remains latent more than a few months. If these propositions have been established, it clearly follows that by destroying every dog in which the disease should break out during strict quarantine, not only would the propagation of the malady be prevented, but the absolute source of the poison would be entirely suppressed."

It is much to be regretted that these wise suggestions should have remained so long neglected by our sanitary authorities.

No reference has been made either by Mr. Youatt or by Sir James Bardsley to the possible perpetuation of the disease by rabid cats. Mad cats, however, are far less common than mad dogs. A cat is not an aggressively fighting animal. At any time it would rather fly from than resist an attacking dog ; and, if there were no dogs to receive and to impart the disease, rabies would soon, so far as the cat is concerned, die out of its own accord.

I have now set forth to the best of my ability—and perhaps too much in detail—the amount of our knowledge upon a subject which is at present painfully engrossing the attention of the public. I have shown that we possess no valid evidence of the spontaneous origin, now-a-days, of rabies in the dog or in any other animal; and that hydrophobia owes its parentage exclusively to the poison furnished in the first instance by the rabid dog, or by rabid animals of the same species with the dog.

I propose next to fortify my position by pointing out that large portions of the habitable world, abounding in dogs, are now, and have always been, entirely free from those dreadful twin pests, rabies and hydrophobia.

It is my good fortune to have found among my own friends and acquaintances several persons able to give me authentic and valuable information on this subject.

Thus the Bishop of Lichfield, who lived more

than twenty-five years in New Zealand, tells me that he never heard of a mad dog in those islands, and that Bishop Abraham's experience, who was for seventeen years resident there, agrees with his own.

Bishop Macdougall writes me word that there is in Borneo a native dog, like a small jackal, but with a curly rather than a bushy tail, kept in numbers by the Dyaks for hunting deer and pig. These dogs never bark, but when on the scent for game howl with a very musical note. The Chinese settlers also have brought in a dog, resembling the Pomeranian breed. These bark abundantly ; and among the settlers, who eat the puppies as a delicacy, they are so numerous as to have become a general nuisance ; yet during the twenty years in which the Bishop resided at Sarawak he never heard of a single instance of rabies.

I was told a few years since by Sir Henry Young that in Tasmania, of which he was for

seven years the Governor, although there were plenty of dogs, there had been no *mad* dogs, and therefore no hydrophobia. Evidence to precisely the same effect has been furnished to a friend of mine by Sir Valentine Fleming, who left Tasmania in 1874, after a residence there of about thirty-two years. He testifies to the great number of dogs in that colony, and to the total absence of hydrophobia. Again, I have it under the hand of Sir George Macleay, who, with Captain Sturt, diligently explored, for other purposes, all the settlements of what has been well called the "insular continent" of Australia, that the dogs there are troublesomely plentiful, that hydrophobia is utterly unknown, and that rabies has never been witnessed in the dingo, or wild dog of those parts. I have received similar testimony from Mr. Nicholas Avent, of Plymouth, who had the experience of ten years' practice in Victoria.

It had been stated by Dr. Heineken that curs

of the most wretched condition abound in Madeira ; that they are afflicted with almost every disease, tormented with flies and heat, and thirst and famine, yet no rabid dog was ever seen there ; and I have quite recently been assured by Dr. Grabham, whose personal knowledge of Madeira covers sixteen years, and who states that he is well acquainted with the local traditions, and the writings of medical men there, that rabies and hydrophobia are, and always have been, unknown in that island.

About the year 1844, while he was assistant-surgeon to the naval hospital at Malta, Mr. Spencer Wells saw and recognized the first case of hydrophobia which had ever (it is believed) occurred in that island. The woman who was the victim of it had been bitten some two months before by her cat. She had almost forgotten the bite, which had caused her no alarm, so that her imagination could have had no share in producing the disease. The cat afterwards

attacked some other person in the street, and was thereupon killed. It was ascertained with certainty that the cat had been bitten some weeks earlier by a rabid dog, which had arrived at Malta in a ship from Gibraltar, where rabies was prevalent at that time. Mr. Spencer Wells informs me that the Maltese language is an impure Arabic, intelligible all along the south coast of the Mediterranean from Algiers to Egypt. There is no word in the language for mad dog, or for hydrophobia.

Mr. Thomas Bigg-Wither spent three or four years in South Brazil, within the tropics. He and his party hunted there the wild dog and the jaguar (a species of tiger) with a pack of fifty smooth-haired dogs of various breeds, which gave tongue during their hunting. Mr. Bigg-Wither has assured me that hydrophobia and rabies are quite unheard of in that part of the world.

We have seen that conditions of temperature

have nothing to do with the prevalence of these diseases. It is interesting, however, to compare this tropical experience with what has been observed in the opposite climate of the Arctic regions.

Dr. John Rae, who has been good enough to write to me on these subjects, was for twenty years in the Hudson's Bay Company's territory, ten of which years were spent at Moose Factory, on the shore of Hudson's Bay, and a year or two each at various other stations as far north as the Arctic circle, at all of which places dogs in greater or less number are kept for sledging purposes, yet he cannot remember to have seen or heard of a single case of the diseases in question, either in dog or in man. My knowledge, Dr. Rae says, of the Eskimos is much more limited, for although I have seen these interesting people at various parts of the Arctic coast, I have wintered only twice among them, on both occasions at Repulse Bay. But I never

saw or heard of any disease resembling hydrophobia.

My distinguished friend, Admiral Sir George Back, who is cognizant of Dr. Rae's testimony in this matter, fully confirms it by his own experience gathered in five expeditions of discovery to the Arctic regions during a period of eleven years' service.

A portion of Dr. Rae's information, although it has no direct bearing upon my main purpose, may prove as interesting to my readers as it has been to myself:—

“ The food of the dogs in Hudson's Bay consists wholly of meat or fish, or of a mixture of both ; meat being the chief diet in the prairies, while fish are almost universally given (except when on a journey) in other parts of the country. In the summer, when not required for sledging, the dogs are sent in charge of a man or two to a fishery, where they can be well and cheaply fed. The usual ration is a fish weighing 3 or 4 lbs., eaten raw. The best and lightest food for the dogs when at work is dry buffalo or deer meat, about 2 or $2\frac{1}{2}$ lbs. of which is a day's allowance.*

* All those who have been personally conversant with

Colonel Home, C.B., an Engineer officer living last year for some months at Constantinople, informs a friend of mine that, having a horror of hydrophobia, he made repeated and special inquiries there, and was assured that no instance of the disease was ever known in that city. He describes the scavenger-dog "as being in temper and feeling a dog, but his appearance is that of a wolf—a dog in wolf's clothes. He has short pricked ears, and a bushy tail which looks as if it had lost a couple of joints. Usually he is of a foxy hue, but occasionally dark and almost black on the back, where a sore is often to be seen. His fur is very thick and shaggy, and he is of the same size as a wolf." There are in the Zoological Gardens two Syrian wolves which present an exact *fac-simile* of the Constantinople scavenger-dog. These dogs, as is well known,

the Arctic sledge-dogs agree in stating that they are subject to a fatal kind of insanity quite distinct from true rabies and accordingly not productive of hydrophobia.

form an important institution in Constantinople, clearing the streets and eating all the offal there to be found. Colonel Home speaks of them as friendly and familiar, and in no way a nuisance, unless some tribe of "civilised" dogs quarrel and fight at night with them or with each other, when the noise they make is fearful. These civilised dogs—country or shepherds' dogs—seem to be badly named, for they are fierce and dangerous, and Colonel Home had to shoot one of two which had pursued and attacked him.

In the *Times* newspaper for the 23rd of October, Mr. Ch. Kroll Laporte, of Birkdale Place, Southport, writes that he never heard of a single case of hydrophobia in Africa during travels there extending over two years.

With more time and opportunity at my disposal I might doubtless find further examples of the entire absence of rabies, and therefore of hydrophobia, from certain places; but of this I have surely said enough: and

should it be alleged that in other places, where these diseases had previously been unknown, they have at length appeared, my argument will be only strengthened if I can account for this by special circumstances. To take a single instance by way of sample, in addition to that contained in Mr. Spencer Wells' narrative: I have been assured upon unquestionable authority that Demerara had not within the memory of man been afflicted by the presence of hydrophobia till the year 1872, when rabies was imported by the influx of a large number of dogs from Barbadoes, in avoidance of a tax which had there been imposed upon those animals.

If it be admitted that hydrophobia never occurs except from the reception of the specific poison from a rabid animal, it follows that, rabies being expunged, hydrophobia would necessarily disappear. For this end it would seem to be required that all dogs in the king-

dom should be subjected to a rigid quarantine of several months, as recommended by Mr. Youatt and by Sir James Bardsley. In order to the effectual enforcement of such quarantine, some legislative measures, and the planning and strict observance of certain regulations on the part respectively of our sanitary authorities and our police officers, are presumably pre-requisites. These are matters with which I am neither called upon nor competent to deal. There will be difficulties in the way, but I am persuaded that, if resolutely grappled with, they will not prove invincible.

Here, then, my share towards the accomplishment of the great object of this paper comes naturally to a close. Meanwhile, until the needful steps for the extirpation of rabies can be fully organized and brought into operation, great vigilance will be necessary to keep in check the existing evil. The superfluity of dogs in the kingdom must be abated by the

unshrinking destruction of many; and *all* dogs should be narrowly watched, most especially dogs known to have been bitten or to have been quarrelling, sick dogs, wandering and ownerless dogs, and such as are the playthings of dog-fanciers and others; and all such further measures as may be legal should be taken for lessening the peril and the panic which is at present said to be "frighting the isle from her propriety."

III.

SMALL-POX AND COMPULSORY
VACCINATION.

SMALL-POX AND COMPULSORY VACCINATION.



I.

ONCE more, and for the last time, I ask permission to preach to the British public from the pulpit of the *Nineteenth Century*. On two former occasions I have advocated methods for abolishing from this island two fertile sources of death and terror, with their long inevitable train of misery, ruin, and bodily disablement—I mean the group of zymotic diseases on the one hand; the pests of hydrophobia and rabies on the other. Hitherto I have preached with no apparent practical success. If a tenth part of

the energy which, on behalf of the pecuniary interests of our agriculturists, has twice been successfully put forth in extinguishing the cattle plague, had been expended in the service of the far higher and wider sanitary interests of the general community, the objects of those former writings might, ere now, have been well-nigh effectually secured.

Things being, however, as they are, I propose in the present paper to bring before my readers, as briefly as I may, some portions of the history of the two kindred subjects, small-pox and vaccination—of small-pox, the most hideous, loathsome, disfiguring, and, hydrophobia excepted, probably the most fatal also of the various diseases to which the human body is liable ; and of vaccination as a preventive of small-pox.

In the first of my papers published in this Review, the grounds were stated for believing that, whatever may have been its primary origin,

small-pox never occurs in the human subject nowadays but as the result of contagion.

Before I treat of vaccination I must notice an expedient which was adopted previously to its discovery, and which contemplated a mitigation of the severity and danger of small-pox. This expedient was the inoculation of persons in health with some of the matter contained in the pustules which characterise that disease. It is notorious that small-pox may be thus imparted by the insertion of a very minute quantity of such matter just beneath the surface of the skin. This is not surprising ; what is really surprising is that the disease so imparted should be much milder than if it were contracted in what is called the “natural way,” by breathing an atmosphere charged with the contagious poison. Why it is so I shall not now stop to inquire. It is enough for unprofessional readers to know that there are two forms of small-pox—one in which the pustules are numerous enough, and

closely set enough, especially on the face, to run many of them into one another; the other in which they are separate from each other and distinct: the *confluent* and the *discrete* forms of small-pox. Now the confluent form is much the more dangerous of the two, and the peril belonging to it occurs chiefly upon the eleventh day of the disease, when what is known as the "secondary fever" sets in. As a rule, in the inoculated disease the period of incubation is shorter, the pustules are seldom numerous, and still more seldom confluent, and the secondary fever is generally slight or absent.

The story of the introduction of the practice of inoculation into this country is trite; but it is interesting and important enough to bear rehearsal even now.

Putting aside vague traditions of the antiquity of this method in China and Hindustan, it is certain that we learned it from the Turks, among whom it was in use at the very begin-

ning of the last century, and perhaps somewhat earlier. In 1713 Dr. Emmanuel Timoni, an Oxford graduate who had settled in Constantinople, wrote to Dr. Woodward, in London, giving him an account of the new process, and testifying to its success. This account was communicated to the Royal Society, and published in its *Transactions* the following year. In 1715 Mr. Kennedy, an English surgeon who had travelled in Turkey, gave similar information to the English public in his *Essay on External Remedies*; and in the *Philosophical Transactions* for 1716 you may see a notice of the same process as described by M. Pylarini, the Venetian consul at Smyrna. Strange to say, these statements were neglected, or had no practical result. We owe the actual introduction of the practice of inoculation to the good sense and courage of an English lady, whose lively letters have taken their permanent place in our country's literature. Lady Mary Wortley Mon-

tagu, the wife of our ambassador at the Ottoman Court, writes thus from Adrianople in the year 1718 :—

“ The small-pox, so fatal and so general among us, is here entirely harmless by the invention of *engrafting*, which is the term they give it. Every year thousands undergo the operation ; and the French ambassador says pleasantly that they take the small-pox here by way of diversion, as they take the waters in other countries. There is no example of any one who has died in it ; and you may believe I am well satisfied of the safety of this experiment, since I intend to try it on my dear little son. I am patriot enough to take pains to bring this useful invention into fashion in England.

In fact she recommended it by her own example. The first person inoculated with the small-pox in England was her daughter. Then the child of a physician, Dr. Keith, who had visited Miss Wortley ; afterwards some felons who had been condemned to death, and who were pardoned on condition of their submission to the experiment ; and at length two daughters of the Princess of Wales—namely, the Princesses Amelia and Caroline. But the practice was not thoroughly es-

tablished, nor properly appreciated by the English public until the middle of the century.

The efficacy of inoculation in saving the lives of individuals and preventing deformity was signally great. The mortality in the natural small-pox was estimated at one in five. It is really higher. Mr. Marson inferred from the records of the Small-pox Hospital that the natural small-pox destroyed about one-third of all whom it attacked. Dr. Gregory says, "the average number of deaths at that hospital was only three in a thousand." The National Vaccine Board speaks decidedly of "one in three hundred" as the proportion of the inoculated that "will surely die from the operation."

The true value of inoculation, upon the whole, cannot even be considered equivocal. To all individuals doomed to have small-pox its advantages were indeed great and obvious: to the community at large it was a grievous evil.

By carrying the virus and the disease into every village throughout the length and breadth of the land, the practice of inoculation multiplied the foci and enlarged the sphere of contagion, insured the disease to all who were subjected to the operation, and diminished to all who were not the chance of escaping it. In truth, the total mortality was greatly increased by the process. Subsequently to the discovery and establishment of vaccination, of which I am presently to speak, inoculation became unjustifiable, except under very special circumstances.

That special circumstances might and may arise to warrant the operation, take the following instance which Professor Gregory of Edinburgh was in the habit of relating on the authority of a naval surgeon. Small-pox was introduced among the crew of a man-of-war in a tropical climate, where no vaccine matter could be procured. The men were almost all unpro-

tected by vaccination. Sixteen of them took the disease in the natural way, and nine of these, or more than one-half, died. Of 363 who were inoculated, under the disadvantages of a hot climate and no preparation, not one perished.

II.

THAT a disorder communicated to the human animal from one of the brutes should protect the former against the contagion of small-pox is one of the most interesting facts in the whole history of medicine. How glimpses of a truth so remarkable were first revealed to the casual observation of certain peasants, and how the result of this chance observation was gradually "matured into a rational and scientific form by a mind deeply imbued with the best principles of sound philosophy," I have not space to explain in detail. And it is the less necessary that I should do so, as the whole subject has been thoroughly narrated and dis-

cussed by Dr. Baron in his interesting biography of Edward Jenner.

Dr. Jenner found among the great dairy farms in Gloucestershire a popular belief that no person who had had the cow-pox (an eruptive vesicular complaint communicated from the udder of the cow to the hands of the milkers) could "take the small-pox." Satisfied by inoculating with small-pox matter several persons who had had the vaccine eruption that it was not an unfounded notion, he at length conceived the great and happy idea of propagating the cow-pox from one human being to another, and so preventing in all cases the perilous and disfiguring distemper of small-pox, which he hoped might thus be finally expelled from the earth.

Dr. Jenner next set himself to trace, if possible, the origin of the disease of the cow. First, he found that it was peculiar to certain dairies; then that in these dairies *men* were employed in milking. Following up this clue,

he further made out that these men had also the charge of the farm-horses. Next he learned that the teats of the cows generally began to present the specific eruption at that time of the year when a complaint called the "grease" chiefly prevailed among the horses. Hence he concluded that the malady was conveyed to the cows by the hands of the men who had been dressing the heels of horses affected with the "grease." Subsequent inquiries established the fact that the disease, which in the horse corresponded with, and produced the specific effects of, the malady of the cow, was a vesicular eruption having no necessary connexion with the "grease" but extending sometimes all over the animal's body.

The next important step in this most interesting investigation was to determine whether the vaccine disease could be transmitted, by engrafting, from one human being to another ; and whether, if so transmitted, it retained its

protecting power. The 14th of May, 1796, is reckoned the birthday of vaccination.

“On that day matter was taken from the hand of Sarah Helmes, who had been infected by her master’s cows, and inserted by two superficial incisions into the arms of James Phipps, a healthy boy about eight years old. He went through the disease apparently in a regular and satisfactory manner ; but the most agitating part of the trial still remained to be performed. It was needful to ascertain whether he was secure from the contagion of small-pox. This point, so full of anxiety to Dr. Jenner, was fairly put to issue on the first of the following July. Variolous matter, taken immediately from a pustule, was carefully inserted by several incisions, but no disease followed.”

The truth is, that the vaccine disease is really small-pox, rendered mild by passing through the system of the cow. The great object of inoculating the small-pox was to produce a benignant form of that disease by diminishing the number of its pustules. The cow-pox diminishes the number to one, and while it reduces the severity of the disease to a *minimum*, it absolutely takes away its power of propagating itself except by direct engrafting of the visible

virus. The disorder is not sufficiently intense to taint the air with poisonous effluvia ; it furnishes, nevertheless, the customary protection.

Since the period of which I have been writing, the exact efficacy of vaccination, and the conditions on which that efficacy depends, have been clearly ascertained by repeated observations and trials ; and it is desirable that the public should have authentic and trustworthy information upon these vitally important subjects.

It appears from the official tables published under the authority of the Registrar-General that no less than one-fourth—*i.e.* 25 per cent.—of the whole mortality from small-pox in England and Wales, happens in infants less than one year old, and as much as 11 per cent. within the age of four months. Within the fifth year the proportion reaches the enormous amount of from 75 to 80 per cent. These facts proclaim the necessity of *early* vaccination. It should be as early as is consistent with the

safety of the child. Certainly it should never be delayed, except under special circumstances of excuse, beyond the third, or at most the fourth, month after birth. Dr. Arthur Farre has informed me that he has seen two cases of death from vaccination performed too early—namely, at the age of eight days. He recommends as the proper period three months rather than two, on account of the greater comparative strength at the greater age. But if the contagion of small-pox happens to be present, the infant should be vaccinated immediately after its birth.

It has been found that the protective influence of vaccination, performed upon infants, gradually lessens and wears out, and that *revaccination* becomes necessary. This should be done just after the age of puberty.

On the second or third day after the insertion of the vaccine virus beneath the skin, some slight inflammation takes place in each punc-

tured spot, and what is known as the vaccine *vesicle* is formed. This runs a special definite course which needs not here to be described. Each vesicle consists of, or contains, small cells from ten to fourteen in number.

Certain conditions are demanded with respect to the operation itself. In what I am about to say, I rely confidently on the authority of the late Mr. Marson, who for forty years had the supreme charge of the London Small-pox and Vaccination Hospital.

In order to obtain the full constitutional effect of vaccination there should be several vesicles—say not less than four; and one, at least, of these should be permitted to run its course undisturbed, while the others may be used for further vaccination purposes. The whitish scars left by the operation should present well-defined dotted depressions, corresponding with the cells of the bygone vesicle.

Mr. Marson made it an imperative rule that

every nurse and servant of the hospital should, on entering the service, be vaccinated. In their cases it was generally *revaccination*, and it was never afterwards repeated. These nurses live in the closest daily and nightly attendance upon small-pox patients, and the other servants are constantly exposed to the profuse and concentrated contagion, yet in no single instance during these forty years did any one of these servants and nurses become affected with small-pox. Surely no stronger proof than this can be imagined that revaccination in the adult, rightly performed, is an absolute protection against small-pox, and need not be repeated.

The whole human race, then, is indebted to Dr. Jenner's happy discovery and acute researches for an unspeakable boon and blessing, which, if rightly used, furnishes the means of eradicating from a well-governed community the most loathsome pestilence that the world has known. Where vaccination is, there the

contagion of small-pox need never come. In Denmark the disease had disappeared before the defensive power of vaccination. Chance, and a careless security engendered by the absence of the pest, led to its reintroduction there. I believe a like emancipation has been secured in various other places. It must be obvious to an unprejudiced mind that the facts already stated warrant and demand that the early vaccination of children should be made, by force of law, compulsory upon their parents or guardians.

But upon this fair and priceless charter of safety to humanity there has fallen an ugly blot. On the first introduction of vaccination it was alleged, among other futile and absurd grounds, that it was unnatural and even impious to engraft upon a Christian the diseases of a brute. It is too certain, however, that one really formidable objection to ordinary vaccination does exist—that it may, in some few instances, impart to the subject of it the poison of a

hateful and destructive disease, peculiar to the human species, and the fruit and Nemesis of its vices. Probably this disaster might be averted if in vaccinating care were scrupulously taken to insert nothing but the pure lymph of the vaccine vesicle, to prevent the blood or any of the elements of the blood of the child from whom the lymph is taken from mingling with it; but unhappily we cannot count upon this scrupulous care being always exercised.

On this distasteful subject I shall simply appeal to the printed testimony of Mr. Jonathan Hutchinson, than whom we have not among us a more able and accomplished pathologist. Such facts as he has demonstrated constitute the only rational excuse for objecting to compulsory vaccination.

I can readily sympathise with, and even applaud, a father who, with the presumed dread or misgiving in his mind, is willing to submit to multiplied judicial penalties rather than ex-

pose his child to the risk of an infection so ghastly.

It is in the hope of suggesting a deliverance from this opprobrium to our nation, and perhaps also from the rhetoric with which our House of Commons is annually vexed on the subject of compulsory vaccination, that I have undertaken the present essay.

III.

VARIOUS suggestions have been made in this country, especially by Mr. Ceely, of Aylesbury, for resorting, for a supply of vaccine matter, to its original source, the cow; but with no abiding good result that I am aware of. In the year 1868 a systematic plan was successfully devised and matured for that purpose in Belgium. Of this plan, which is patronised by the Government, and administered under the direction of Dr. Worlomont, Secretary to the Medical Congress of Brussels, I proceed to give a brief description.

A healthy and well-nourished calf, about three months old, is *hired* from a butcher, and vaccinated in the usual way, on its shaved abdomen,

in about sixty places. Upon the punctures thus made *vesicles* form, as from ordinary vaccination in the human body. These vesicles run their due course, and the vaccine virus which they contain is ripe and fit for use about the fifth or sixth day of that course—for use, namely, from the living animal in direct vaccination, and for collection in a fluid state into tubes, or in a dry state on ivory points, for the purpose of vaccination which is indirect. After seven days the calf is returned to the butcher, none the worse for what has happened.

The accuracy of this short detail has been warranted to me by Mr. George Critchett, who during a visit to Brussels closely inspected the whole process. It has also been personally witnessed by Mr. Colam, and by him acquitted of all imputation of cruelty to the animals concerned.

Now are these vaccinations effectual? Does the infection "*take*?"

It is stated in Dr. Worlomont's Report that when the operation is rightly performed, each puncture with lymph from the living animal produces a *vesicle* which may vie with the best that result from vaccination with human lymph. As to the virtue of the dry vaccine matter carried on ivory points, the following is the outcome of the experience of thirty-six of the most eminent practisers of medicine in Belgium. Out of a total of 500 cases, 479 were successful, or 96 per cent. Of 5,425 instances of revaccination there were 3,419 successful, or 63 per cent.

He adds, upon what authority I know not, that the best vaccinators in England claim only 95 successes in 100 cases of vaccination with dry human lymph on ivory points.

Of the protective power of the Brussels vaccinations, Dr. Worlomont cites the following conclusive evidence :—

Among more than 10,000 children vaccinated

at Brussels from 1865 to 1870, and living afterwards amidst the terrible epidemic of small-pox which alarmed everybody in 1870 and 1871, there was not known a single instance of an attack of small-pox; and the same absolute immunity from that disease was enjoyed by the far greater number of *revaccinated* persons who also lived within the sphere of the contagious epidemic.

The quantity of vaccine matter generated in the calf may be judged of from the estimate that a single animal will supply enough for similarly vaccinating its successor, and also for performing the operation upon a regiment of 400 soldiers.

The *plant*, so to speak, provided for these actual proceedings consists of two waiting-rooms, one for applicants who pay for the vaccinations, the other, and larger, for those who, or whose children, are vaccinated gratuitously. There are also two other rooms, appropriately

furnished, one of which communicates directly with a stable capable of containing six calves ; and this stable opens upon a portion of the Zoological Garden at Brussels, and can therefore be kept sweet and perfectly clean.

The necessary expenses of the whole establishment, which are defrayed by the Government, are but moderate in amount.

Surely similar institutions might be formed in London, and in all the large towns of this rich kingdom, whereat the general public might obtain vaccination from the baby or from the calf at their pleasure, while *compelled* vaccination should in all cases be from the calf alone. It seems to me that all this could be effected, under the authority and control of the Local Government Board, without derogation from the status or the emoluments of our licensed vaccinators, and therefore without opposition on their part.

There are sufficient grounds—besides the

avoidance of the risk, small or great, of disseminating with the vaccine virus the obnoxious human poison—for seeking a fresh source of the former. Dr. Jenner himself was not without apprehension that the repeated transmission of the vaccine lymph from one human being to another, without recurring to the cow, might impair its virtue. Long ago I had for one year a seat, as the Senior Censor of the College of Physicians, at the National Vaccine Board, and I then had opportunities of satisfying myself that lymph which had been transmitted from person to person ever since the time of Jenner continued to produce what appeared to me a very perfect vesicle. But Mr. Simon has stated some strong reasons for suspecting that the “occasional impermanence of protection may depend upon impairment in the specific power of vaccine contagion—an impairment arising in the transmission of that contagion through many generations of men.” Dr. Gregory and Mr.

Eastlin also have recorded facts showing "that the vaccine lymph, by passing through the bodies of many persons, loses, in process of time, some essential part of its activity." This opinion gathers force from a very curious result of the experience furnished by the Prussian army. It appears that where the vaccine supply has seldom or never been renewed from the cow, the proportionate resusceptibility of vaccine disease at a given age (and therefore, it may fairly be presumed, the susceptibility of small-pox also) has undergone a progressive increase. "And," argues Mr. Simon, "it is difficult to conceive how the infantine generations of a country could, crop by crop, successively derive less permanent constitutional impressions from vaccination, unless the efficient cause of those impressions—the vaccine contagion itself—had year by year undergone enfeeblement of its powers."

Mr. Marson's testimony is in concordance with

all this. He said that in the course of years, vaccine lymph, by passing many times through the human body, became *humanised*; that the cicatrices, according to his own experience, were not so good as they had been formerly; and that the mortality after vaccination, estimated on a large scale, had shown a considerable increase—viz. from 6·56 per cent. for twenty years, from 1836 to 1855, up to 9·2 per cent. out of 1,958 cases, for the years 1863 and 1864.

The President of the College of Physicians, Dr. Risdon Bennett, informs me that in the year 1870 he was in St. Pétersburg, and saw in the Foundling Hospital of that city some rooms fitted up for vaccinating upon and from the heifer, and witnessed and admired there also the decency and strict attention to order and cleanliness with which the whole process was conducted.

I have reason to believe that in other coun-

tries the results of vaccination from the calf are less uniform and sure than those met with in England ; but this does not affect my sole object in writing this short essay, which is to put an end to the only valid objection that has been or can be advanced against vaccination generally—namely, that when the vaccine lymph is conveyed from a human arm it may carry with it to the person vaccinated, the poison of a dangerous and hateful malady.

THE END.

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